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THE FUNCTION OF CLOTHING AND OF BODILY ADORNMENT[1]

By HERBERT C. SANBORN, Vanderbilt University

One of the popular questions of the hour concerns the style of woman's dress. For woman herself this is said to have furnished themes of perennial interest well beyond the ken of psychologist and philosopher. But for the latter, too, the general problem of clothing has been at various times a puzzling subject of reflection. At the present moment it may have for them a more especial attraction, inasmuch as the short skirt and the short hair of the present generation have come to be confidently linked by reformers with certain other alleged short-comings said to have developed *pari passu*. Proceeding on the usual assumption that clothing and modesty stand in a one-to-one correspondence, it would be indeed easy to arrive at the conclusion that the recent shortening of the skirt is the explanation for an alleged moral turpitude, or vice versa—an opinion widely held by the man in the street.

If we may credit newspaper reports, this view of the matter has been the ground for attempted regulation of dress and head-dress of employees in certain American business houses, the theme for occasional sermons from the pulpit concerning "the sacredness of the body," while women have been forbidden to enter certain churches unless attired to suit the regulations of those in charge. Moreover, we are informed that far from the influence of Puritanism, near the region where Spartan youths without moral jeopardy wrestled naked with naked Spartan girls and Greeks in general were lightly clad, the maid of Athens has had the proper length of her skirt prescribed for her by some modern Draco. On the other hand, however, it is perhaps not absolutely unworthy of mention in passing that other reformers in Europe, reacting against what they call effete civilization, are seriously developing cults for the abolition of clothing altogether

[1]Read at the annual meeting of the Southern Society for Philosophy and Psychology, held at the University of Kentucky, April 3, 1926.

except for outdoor protection against the weather. Apparently there is a clothing problem that is not settled and will hardly be settled by the present article.

The view that clothing is a sign of modesty is at least as old as the ancient account of the predicament of our first parents, which is paralleled by the traditions of certain Indian tribes of a period when no clothing was worn by their ancestors.[2] Such accounts seem to suggest that the feeling of shame is an acquired habit; but together with other data they at any rate bear witness to the fact that man has long wondered why he began to wear clothes. As a result of such speculation, especially in recent times, attempts have been made to find some plausible explanation for the custom that shall rest on a basis of scientific fact. When we turn, however, to the conclusions drawn from a consideration of the data furnished by anthropological research we find here as elsewhere varying and even antithetical interpretations.

With investigators like Westermarck, Hirn, and Ernst Grosse, theory has even swung to the opposite extreme of the view which finds in natural shame the impulse to clothe the body, arriving then at an interpretation which imputes the first use of clothing to coquetry or exhibitionism. At first sight such a startling view may seem all of a piece with the perversity that has succeeded, as Kuno Fischer said, in producing "a merciful Nero, a wise Caligula and a virtuous Henry VIII," but the mere fact that it is subscribed to by men of the type indicated makes it in any case worthy of careful consideration.

The genetic basis for the theory is found in an alleged sexual apathy on the part of the male and a certain coquetry on the part of the female, claimed to be practically a universal phenomenon of wild animal life. These are held to be an explanation for the secondary sex characteristics and the courting behavior of some animals, which indeed also led Darwin to his theory of animal aesthetics and the auxiliary hypothesis of sexual selection. Moreover the theory seems to bring these facts into line genetically with the use of various kinds of ornamentation in savage and civilized life.

The function of jewelry is said to be the attraction of attention to the beautiful hand, arm, or head, rather than to the ornament itself; and this is so much the case that the effect of excessive ornamentation is to defeat its purpose by holding the attention too exclusively; a large diamond, as Hirn says, prevents the beautiful fingers and hand from being noticed, so that women of taste have a natural repugnance for large rings, stones, bracelets, and ear-rings. In the case of primitive peoples, accustomed to being naked and with whom clothing, at times at least, is indistinguishable in its function from ornament, it is alleged that the effect of certain articles worn is to call attention emphatically to the pudenda that might otherwise remain unnoticed or at least sexually ineffective.[3]

[2] Cf. E. Westermarck, *The history of human marriage*, 5th ed., I, 540.

[3] Y. Hirn, *Der Ursprung der Kunst*, 1904, 205; cf. also E. Selenka, *Der Schmuck des Menschen*, 1900, 20; and Heinrich Schurtz, *Grundzüge einer Philosophie der Tracht*, 1891, 141

Westermarck utilizes in his exposition vast resources of material gleaned from the accounts of explorers and anthropologists in all parts of the world concerning the self-decoration of savages; and he lays especial stress on the claim that there is no people so rude as not to take pleasure in ornament though destitute of almost everything that civilized people regard as the necessities of life. Primitive man attaches to his body all the ornaments he can secure, many of which, on the testimony of some savages, function at present as a means of sexual allurement.

In order to make themselves agreeable to the opposite sex, men and women paint and tattoo themselves, submitting themselves to many painful operations in order to fasten rings, sticks, shells, pieces of fat meat and other objects in the nose, lips, ears, and the pudenda, the ornamental value of which is alleged to increase with the number and size of the objects worn. Sometimes the teeth are knocked out and sometimes they are blackened, browned, or sharpened, the purpose being, according to some of the savages, to make the mouth of the possessor as different as possible from that of an animal; and only the possessors of such teeth are considered marriageable. Similarly painting, dressing, and ornamenting the hair in various ways is said to serve as a sexual stimulant, whereas in many races short hair, especially with women, is a symbol of chastity. Self-decoration is in many tribes more prevalent with the men than with the women, a fact which is supposed to parallel the secondary sex characteristics of the male with birds and other animals. The contrast between this situation and civilized life of the present day is explained in the theory by the fact that in savage life males predominate, whereas in civilized life the opposite obtains. The intimate relationship between self-decoration and clothes brings about a mutual substitution of function, such that clothes readily become decorations while mere decorations have also developed into clothes.

The origin of clothing may, perhaps, according to Westermarck, be traced to several different sources, the most obvious of which is the desire to protect the body from frost and damp. The practice of veiling the face in Mohammedan countries he takes to be a form of protection against the evil eye and he presents other evidence for his contention that the covering of the pudenda with peoples otherwise naked is due in some cases to this same motive; in others it may come from the desire to secure protection against the attacks of insects or to prevent injuries to the organs. Still another motive seems to be furnished, according to Havelock Ellis, Lombroso, and Ferrero, in the desire to forestall the arousal of a feeling of disgust at certain secretions, which is, according to Lombroso, the sole form of modesty among prostitutes to-day.

While admitting, however, that a thing may be invented for one purpose and afterwards serve for another and that savages who now look upon clothing as ornament may not perhaps have done so from the very beginning, Westermarck believes nevertheless that, in many cases at least, the pubic covering was originally adopted as a sexual lure. He quotes in support of this the affirmative statements of some savages, and calls attention to certain practices among primitive races that seem to bear out his contention. Among the Saliras, on the

authority of Lohman, only the harlots clothe themselves. Furthermore, clothing is not the only way of attracting attention in the way desired, since in many of the Pacific islands the pudenda are tattooed and mutilated by subcision and introcision ostensibly with the same intent. Westermarck diverges from Ellis in his interpretation of the apathy of the male, regarding coyness of the female not as indispensable to tumescence but rather as promoting by delay more complete and abundant secretions of the sexual glands. Furthermore, he does not claim that peoples who attach no idea of shame to the exposure of the entire body are devoid of modesty, quoting on the contrary Kerr Gross to the effect that "as a general rule, in Africa modesty is in reverse proportion to clothing."[4]

In contrast with this interpretation of clothing stands that of Schurtz, Ratzel, and Wundt which tends to confirm the popular view that clothing is originally due to a certain natural sense of shame. The essay of Schurtz, upon which Wundt draws and to which Westermarck gives some consideration, is worked out in opposition to the position taken by Von Schleinitz and Rohrbach. Von Schleinitz, from his study of the natives of New Britain, where the women though practically nude are decent in their behavior, had come to the conclusion that the impulse to clothe the body had originated in the need for protection against the exigencies of climate, and perhaps in the impulse to adorn the body. He believed that modesty had been developed by the custom of wearing clothes through a long period of time. Rohrbach had already noted the fact that the first coverings are for the middle of the body, claiming that this was due, not alone to the feeling of modesty but rather to the desire to adorn the body; hence women are more often thus adorned than men. Peschel had also traced the origin of clothing to aesthetic motives.

In the opinion of Schurtz, the purely inductive method of many investigators leads to no results and must be supplemented by a deductive ethnology based upon the results of introspection. His study is concerned chiefly with the African races, but he also draws on material contributed by ethnologists from other parts of the world. With the majority of investigators he is in agreement that clothing proper, as distinguished from clothing used for ornament, begins as a covering for the pudenda, apparent exceptions to this being found among races representing declines in civilization. Clothing proper is known to every people; and even where reports at first sight seem to indicate that there are races living in a condition of absolute nudity, a closer investigation reveals that a small group among them—usually the married women—have some sort of clothing. Moreover, such races have a latent sense of shame that manifests itself with respect to strangers, even though it may be quiescent within the circle of

[4]Westermarck, *op. cit.*, I, 419-571. *Sexual shame* is defined by him as "shame caused by idea of the sexual function or anything that is apt to lead to such an idea;" *sexual modesty* is "the fear of or the tendency to avoid anything which would arouse sexual shame."

the tribe itself, just as in civilized life the sense of modesty is stronger in its manifestation toward those outside the family circle than toward members of the same family. With Ratzel he is in fundamental agreement that modesty is natural and universal, and finds that the covering for the pudenda remains as the last vestige of clothing with the very lowest declines of civilization.[5]

The view that clothing originates in a desire to protect the body from cold will not hold for races living beneath the tropics and those who wear the articles in question at present do not usually regard them as protection. Moreover, we have drawings in the caves of Perigord which show that man was not forced even by the cold of the Diluvial Period to put on warm clothing. While a report of Von den Steinen seems to show that the garments in question may have served secondarily as a protection against the bushes in roaming through the thickets or against the attacks of insects, the theories which take these as primary functions are refuted by the fact that the articles themselves are quite unsuitable for the purposes alleged. In parts of Africa in the temperate zone the factor of climate may, however, have led to an *increase* in the amount of clothing worn.[6]

The close relationship between clothing and sex is revealed most distinctly through the fact that all important events of sex life go regularly hand in hand with change in dress. In the case of most savage tribes clothing is not assumed until the beginning of puberty; and then a distinction is made in the dress of the sexes, prescribed strictly by custom. With the advance of civilization, however, and the advent of marriage this stage comes to be considered of less importance, the emphasis being then laid on the distinction through dress of the married and single female members of the given group. According to Schurtz, this fact throws a flood of light on the psychological significance of clothing, which in many cases is plainly seen to be a symbol of the married state. With some races the expression, "he gave her a dress" means that a man has married a maiden and not a widow,[7] while on the island of Tahiti the chief ceremony in the marriage rite consists in the groom throwing the birde a piece of cloth. With other races the woman who has borne a child wears an additional apron. The feeling of modesty is a necessary accompaniment of social organization and develops concomitantly with the development of marriage, that is, with exclusive sexual possession, clothing being the outward sign of this process.[8]

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[5]Schurtz, *op. cit.*, 1 ff., 42, 45, 48.

[6]*Op. cit.*, 11 f., 39, 111, 112, 116, 120; cf. also Selenka, *op. cit.*, 58.

[7]The suggestion of something similar is perhaps to be found in the antithetical expression, "donner une cotte verte à une fille," the origin of which is obscure.

[8]*Op. cit.*, 13 ff., 31, 33, 44, 54, 66, 72, 75 ff., 95, 103, 137.

The phenomenon of jealousy in the case of gregarious animals endangers temporarily the very existence of the herd through the violent battles of the males in the mating season; and if this situation were continued for more than a brief season the herd would be defenseless before its enemies. The fact that for human beings there is no especial mating season would make the existence of human society likewise impossible, unless man in his development had been able to succeed in uniting the gregariousness of the herbivora with the monogamy of the carnivora. The primary cause for the initial stages of clothing is the jealousy of the male, who imposes his will upon the woman owned by him. The religious rites used to sanctify marriage might easily seem to precede this, but religious commands are regularly customs already sanctioned by the needs of practical life. It is even possible to admit that clothing proper may have been used first of all as adornment or as protection from cold during sleep, and also that the sense of modesty has been subject to a gradual intensification without committing oneself to the thought that it has been created—even in the case of races living under the tropics—by the use of clothing for some other purpose. Modesty in its development extends normally from woman to man, which explains why woman regularly wears more clothing than man in primitive society. When exceptions to this rule have been advanced, it has been shown that in some races man has taken advantage of his superior position to allow the woman only the scantier apparel, so that in some cases leaves and foliage constitute her sole attire. With the development of civilization the use of clothing gradually extends to other parts of the body; and in the case of these the feeling of modesty may well arise as a secondary phenomenon.

Thus clothing may come to be worn behind instead of in front by tribes living in huts into which they must crawl, inhabitants of the desert may come to transfer the feeling of modesty to the exposure of the face which was veiled originally in the desert to prevent thirst. Furthermore, among many degenerate peoples that appear at first sight to be absolutely naked, there are various forms of symbolical clothing, due to poverty, laziness, and negligence. Thus a mere string of leather, cloth, or pearls, about the waist or about the breast may symbolize a real garment previously worn, while the true meaning of the symbol is unknown to the wearer. With such symbolization there goes hand in hand in many races the tattooing of the body with designs that simulate the garments in question. Closely allied with this, we often find oiling and greasing of the body so that it becomes covered with a layer of dirt or ashes that serves secondarily as a protection against the weather and insects. Even with this poorest of garments there is a differentiation for the various classes in the tribe. Painting the body also serves to symbolize the painted clothing common among certain races, and is sometimes resorted to for medicinal effects.[9]

Under the impulse to adorn the body, more and more clothing comes to be worn and thus develops into money, or becomes a sign of the wealth of the wearer. In connection with this arise many customs later not understood, such as the removal of clothing in the presence of a superior, the meaning being that the inferior (slave or subject) must not in any way conceal himself or his possessions from his master; the master or potentate, on the other hand, retains all his clothing, or is concealed behind a screen, or the subject is obliged to prostrate himself with his face to the ground and must not look directly at his superior.

[9] *Op. cit.*, 20-83, 110, 121.

Out of this grew the practice of secluding the monarch, as in the case of puppet kings. As a development of the practice of removing all the clothing, there comes later a merely symbolical removal of it in the taking off of certain articles, such as the shoes or the head-dress; from the latter has come our present custom of removing the hat in greeting a person (found also among those negroes in Africa who wear head-dress) and even, as Schurtz believes, the decolleté dress which was first worn on solemn occasions at court.[10]

The view of Peschel that the veiling of the pudenda occurs from aesthetic motives is rejected by Schurtz because *naturalia non sunt turpia*, and he attempts to reënforce this by showing that while it is difficult for Northern races to assume a purely aesthetic point of view with respect to sculpture, the Greeks would not tolerate clothing in their statuary *except* as ornament.[11]

Wundt, relying on the work of Schurtz and Waitz and tacitly assuming with most investigators that human life originated in some tropic region, finds that clothing is unnecessary for protection in a warm climate; he believes that coverings for the pudenda are to be explained by the "specifically human feeling of modesty." Tattooing and mutilations have originally a religious significance, but later came to be transferred to clothing worn because of climate. One of the chief differences between the savage and the civilized man to-day is to be found in this distinction between direct and indirect ornamentation of the body; but there are transitional stages from savagery to civilization where both forms of ornamentation exist side by side. Even among civilized races traces of the direct ornamentation are found in the artistic adornment of head and beard with the Assyrians, in the painting of the hands and face with the Egyptians, the tattooing of the hands and face with the Arabs, the braids once worn by European men, and finally in the earrings, finger-rings, and hairdress of the modern woman. The first step toward the abolishment of barbaric and Oriental ornamentation was taken by the Greeks, but Oriental influence made itself felt again in the picturesque dress of the mediaeval period to be succeeded by a return to Grecian simplicity during the Renaissance. This was followed by the wigs and braids of the Barocco and more recently, in the modern period, by practical utilitarian clothing, which has little or no regard for aesthetic values.

Two transitions in this development according to Wundt are of ethical import: first the passing of ornamentation from man to woman, which symbolizes her elevation from a condition of slavery (where she needed

[10] *Op. cit.*, 107-142. [11] *Op. cit.*, 12 f., 142.

only the simplest possible working costume) to a position of comparative equality with man; and, secondly, the transference of ornamentation from the body to the immediate household environment. The latter symbolizes the comparative equalization of classes, so far as bodily appearance is concerned, except in the case of various kinds of officials. The evolution of cloths mirrors the evolution of morality, and the aesthetic and ethical interpenetrate each other, revealing at one and the same time the aesthetic taste and the ethical character of a given people.[12]

Ernst Grosse comes to the support of Westermarck from the point of view which affirms that ornament antedates clothing. He emphasizes the prevalence of red as a secondary sex characteristic and as a color preferred by primitive races, claiming in opposition to Gerland's religious interpretation of tattooing and scarification that these have an identical aethetic function, tattooing being confined to the yellow races and scarification to the black. Following Lippert he finds that the location of bodily ornament is structually determined by parts of the body like the neck and especially the hips which offer natural support to collars and girdles for tools and weapons. The fact that the covering for the pudenda is not universal and not adequate, together with the further fact that our children have no natural shame, seems to him proof that the theory commonly accepted by anthropologists is false.[13]

The significance of clothing in its strictly ornamental function was first emphasized, so far as I am aware, by Lotze in the *Mikrokosmus*, where he develops a line of thought, worked out later by Friedrich Theodor Vischer and Lipps in the doctrine of *Einfühlung*, and especially by Lipps and Selenka with respect to the significance of bodily adornment. After discussing the intimate manner in which we feel ourselves to be present immediately in the various tools and instruments used by us, so that we seem immediately to touch the paper where the pen is in contact with it, to cut the meat where the knife passes through it, etc., Lotze takes up for consideration the many kinds of bodily ornamentation both from the point of view of the effect on the wearer and on the observer. Except perhaps in the case of colors, he observes, the satisfaction in ornament does not reside in mere vanity aroused by the admiration of others. It consists rather in a special enhancement and refinement of self-feeling experienced by the wearer. Head-dress and foot-wear give us a satisfying sense of increased extension in the vertical. With the movements of the head to and fro we experience an extension of our personality into the very top of the high hat. Similarly with the cane which we carry or swing and with which we touch various objects. Tassels, ribbons, and

[12]W. Wundt, *Ethik*, 1912, 152-161; *Völkerpsychologie*, Bd. 3, 1908, 176-189, 219, 225.
[13]Ernst Grosse, *Die Anfänge der Kunst*, 1894, 52-110.

other fluttering forms of bodily adornment give a delightful experience of being in rapid succession at many points of space, making doubtless for a certain animation and vivacity in the wearer. Children sometimes tie onto themselves a tail that they drag along on the ground or wave in the air behind them for the sake of this enhancement of self-feeling. The greater or less tension of our clothing is felt as though it were a quality of our very selves. The wearer of a corset seems to be the possessor of the strength and elasticity of the garment, just as the wearer of armor assumes the qualities of the metal. Veils and light delicate fabrics worn by women produce in them corresponding experiences, that are reflected in their behavior, and all of these are paralleled by equivalent modifications in the consciousness of the observer.[14] Among other things Lotze suggests that it would be possible to infer much concerning the psychic qualities of various races by a detailed study of their dress; and this Selenka has done under the immediate influence of his colleague Lipps. He does not, however, make any mention of Lotze's discussion, although in one or two places he uses almost identical language and illustrations.

Ornamentation is for Selenka a kind of *Bildersprache* adopted unconsciously by all the peoples of the earth and differing from other sorts of language through the fact of its universality; the identity of meaning in clothing (which is paralleled in its universality only by the expressive mimicry of the human face) he explains as conditioned structurally by the identity of the human form adorned. The language of ornament is only one aspect of a more general language of clothing, a *Bekleidungssprache*, whose elements, with the single exception of hair, have all been borrowed from the world external to the human body. In a long trailing costume a person can not run or move about quickly and hence this sort of clothing means dignity of gait and bearing for both wearer and observer. The crest of a helmet pointing forward symbolizes the charging warrior, etc. Not all symbolical articles of clothing are, however, ornamentation; a tall stiff collar hinders the natural movements of the neck and is a symbol of stiffness and reserve, not an ornament. The language of ornament, in the strict use of the term, expresses only favorable qualities of the one adorned, and originates in a desire to be pleasing; but this impulse and that of vanity must not be regarded as the only motives behind ornamentation. Ornamentation—here he is in agreement with Lotze—exercises at the same time a developing effect upon the wearer, enhancing his self-feeling, influencing his moods, and modifying his behavior.

[14]Hermann Lotze, *Mikrokosmus*, 1869, 199-212.

If it were not for this moral value of adornment, it could never have been developed into the significant language it has become. While other forms of communication are inventions that vary from region to region, ornament is a discovery, characterized by changeless law. The very word 'kosmos' (the foundation of the English word 'cosmetic') means order and law and was consciously used in a double sense by the Greeks, who meant by ornament only lawful ornament as distinguished from decoration or mere finery. True aesthetic ornamentation is always structurally motivated, and this accounts for the universal agreement of the various forms. When the Roman senator chose the long flowing toga, the naked African her rings, the more agile Spanish woman her fluttering ribbons and laces, the Indian the pinions of the eagle, and the Persian the broad tightly-fitting girdle, choice in all these cases was not a matter of chance but was conditioned by an inner necessity.

The clew to an interpretation of all natural ornamentation is to be found in an attentive examination of the human figure, the upright position of which almost exclusively conditions the form of its ornamentation. In contrast with that of other mammals, the body of man is characterized by two directions, the line of the vertical and (in its forward motion) that of the horizontal; these furnish the conditions for the two classes of pendant-ornament and direction-ornament. To these must be added a third kind of ring-ornament, due to the shape of various parts of the body, a fourth kind of augmentative-ornament (epaulettes and wide shoulder collars), a fifth kind of color ornamentation (produced by gems and stones), and a sixth kind of clothing ornamentation in the proper sense. Besides these there are transitional forms. A closely fitting necklace is a ring-ornament, but a necklace large enough to hang down loosely on the breast becomes also pendant-ornament, enhancing at one and the same time the perception of the erect position and the sense of movement and life in the wearer and for the observer, in so far as movements of breathing, etc., become thereby accentuated. Ribbons at rest are pendant-ornament, but with the body in motion they become direction-ornament. A broad shoulder collar provided with fringes is at one and the same time augmentative, pendant, and, with the wearer in motion, direction-ornament. As for Lotze, the ground for satisfaction is found by Selenka in the fact that the wearer identifies himself with his clothes; but he emphasizes also the social aspect of the matter in his statement that there is no tendency to ornament parts of the body covered by clothing and that it would never occur to a person thrown on a desert

island to adorn his person any more than it would occur to him normally to talk to himself.[15]

In agreement with Gottfried Semper, Selenka distinguishes two types of ring-ornament: stiff rings for those parts of the body (arms, head, legs) that change their shape little or not at all during the movement of the wearer; and, secondly, linked or chain rings, worn on ankles, wrists, and neck, which express or symbolize the mobility of these parts. Stiff rings serve to symbolize the expansive life of the muscular parts of the body, and, in the case of head-ornamentation (fillets, head-bands, wreaths, and crowns) to emphasize the form of the head and the color of the hair. Some forms of head-dress, like the broad-brimmed hat, belong to the general class of augmentative-ornament. The aesthetic value of the close-fitting girdle is to give to the wearer a sense of greater strength, while emphasizing the mobility of the upper half of the body in contrast with the rigidity of the hips, but the girdle gets its fuller meaning only with the completely clothed body. Finger-rings have aesthetic significance as local color ornament through the stones in them, while the toe-rings, worn by some peoples, have merely practical utility in protecting the feet.[16]

Pendant-ornament should always be, not rigid but mobile, so that it may adjust itself readily to the true vertical. Thus ear-rings, symbolizing the direction of gravity, must be mobile in order to represent continually the direction of gravity in contrast with the delicately curved lines of throat or neck which manifest their independence of the strict vertical. The aesthetic value of heavy pendant-ornament is enhanced through the fact that it influences the behavior of the wearer by preventing him from making hasty movements, thus putting on him the stamp of dignity. The effect of symmetrical pendant-ornament is relative immobility and in the case of Malay brides it symbolizes an absolute subordination to the will of the husband. One illustration is given of such a bride so loaded down with ornament that she was forced to remain practically motionless for the fourteen days of the wedding ceremonies. The Japanese kimono, which allows the wearer merely to toddle about, possesses a similar meaning. In the case of the moving body, for example in the dance, such ornament serves to emphasize the various positions assumed, and shawls, ribbons, and fluffy fabrics in general give the danseuse opportunity for manifold pendant arrangements. A danseuse in a tightly fitting jersey would be at a great disadvantage and, in fact, ridiculous. Necklaces and girdles when worn loosely as pendant-ornaments give a sense of slenderness to the form of the wearer.[17]

In addition to epaulettes, ladies' trains, head-feathers of the Indian and other forms of augmentative-ornament (all of which may lead to extravagant manifestations), the full beard constitutes a natural kind of augmentative-ornament, lending dignity in most cases to the wearer. When this has been reduced through shaving to a moustache or an imperial we have either a symbolical presentation of the full beard or a form of mimicry. In his discussion of the ornamental function of clothing, in which he follows Lipps very closely, Selenka emphasizes the fact that clothing is ornamental only when it falls into the categories of ornament already pointed out. Not all clothing is ornamental, although it may all be expressive or symbolical. Thus the stiff, high collar of the soldier's uniform means or expresses the rigidity of military discipline, while a large, soft collar may be a true ornament. Various materials, such as silk, plush, lace, gold-brocade have also their specific aesthetic significance. Sometimes ornamentation has merely a physical, sometimes also a spiritual significance, as when the crown draws attention to the head, while signifying mental and social

[15]Selenka, *op. cit.*, 1-14. [16]*Op. cit.*, 14-30. [17]*Op. cit.*, 30-44.

power. The flower in the hair tells, similarly, of the taste and disposition of the wearer, while calling attention to the color and arrangement of the hair itself. Climate, manner of living, environment, racial character, the social position of women, tradition, intercourse, and fashion are all mirrored in the style of ornamentation.[13]

In glancing over the material accumulated in the general field of clothing and ornamentation one is impressed by the fact that the data unearthed are not as unambiguous as might be wished. Moreover, some of the phenomena are in apparent contradiction with other phenomena, and there are contradictions among the investigators with respect to fact that seem to the layman susceptible of elimination. Thus Westermarck, Hirn, and Grosse maintain that there are races living to-day in a state of nudity, while Schurtz and others declare that more careful investigation reveals in all such cases members of the group who are clothed at least symbolically. The assertion that this s true at least for the married women is opposed by the claim that there are Australians regularly naked with the exception of the unmarried women. The emphatic statement of Ratzel, Schurtz, and Wundt that no races are entirely lacking in modesty (also admitted by Westermarck) is met by the contention that there are naked races with no natural shame. The claim that woman is less modest than man, alleged to be shown by the existence of races where the women wear less clothing than the men, is opposed by the explanation that in such tribes (because the position of woman is especially inferior and because clothing has there degenerated into ornament) the women are not allowed to have what little clothing is available; even in such cases it is claimed that the women are more modest than the men and that they resort at times to a clothing of foliage. Scarification is alleged to be for the practical purposes of recording the number of enemies slain in battle, for allowing evil spirits to escape from the body, and for adornment. Similarly, tattooing is explained as due to religious and totemic motives, to use as an amulet and to ornamentation.

The view of Westermarck seems difficu't to accept but almost as difficult to reject entirely. Among its difficulties is the fact that it invites us to believe that modesty or decency as now understood could have developed out of its very opposite; although the doctrine is put forward with the important reservations above noted. In common with most of the theories, too, it assumes that the primitive races now extant represent adequately the ancestors of present civilized peoples, whereas it is possible that most of these so-called 'contemporary ancestors' are merely degenerate or the relics of psychically de-

[13] *Op. cit.*, 44-72

fective stocks, whose failure to progress might be due to an inability to secure such control of their sex life as may have been possessed by the ancestors of the present civilized stocks, granted that there is some relationship between continence and the development of civilization. Many accounts seem to show that, although savages do not regard incontinence as an evil, savage life in general is not characterized by incontinence; but this would seem to be a matter especially difficult to determine for any stage of civilization.

So far as the alleged apathy of the male is concerned, nobody familiar with the behavior of domestic animals will maintain that as a rule it is in their case a matter of fact, and Havelock Ellis, recognizing this, attempts to explain the supposed discrepancy as due to the over-nourishment of the domestic animals. The fact, not mentioned by Ellis, that domestic animals, like the dog, breed oftener and have larger litters than the wild animals with which they are most closely related and from which they are supposed to be descended, tends to make this claim plausible; but in general it is extremely doubtful whether we know enough about the sexual behavior of wild animals to draw the conclusion arrived at by him and others with respect to apathy.[19] So far as the coquetry of domestic animals is concerned it is confined in my own observations to the female and is there rather a form of timidity or fear; mating seems to be quite regularly a conquest by the aggressive males, and such males seem to be the only ones with which some females can be mated. The argument of Grosse with respect to the exciting effect of red for savages, which he connects with the prevalence of red as a secondary sex characteristic, is the popular view; but it has been considerably weakened, so far as animals are concerned, by the experiments of Yerkes on the dancing mouse[20] and by Stratton on cattle;[21] recently, too, there has been furnished a plausible explanation of the secondary characters in the case of birds that brings them into line with the phenomena of protective coloring.[22] The prevalence of red in the painting of the body may be due to the wide geographical distribution of red ochre—the material used—as well as to the further fact that red, like white which is also widely used, stands out very distinctly in contrast with the dark skin of the savage. In the case of savages, it seems quite improbable that

[19] A few animals, birds and squirrels, taken very young and raised by me in captivity have behaved like the domestic animals.

[20] R. M. Yerkes, *The Dancing Mouse*, 1907, 133-177.

[21] G. M. Stratton, The color red, and the anger of cattle, *Psychol. Rev.*, 30, 1923, 321-325; cf. also E. Kittredge, Some experiments on the brightness value of red for the light adapted eye of the calf, *J. Comp. Psychol.*, 3, 1923, 141 ff.

[22] F. M. Chapman, *Handbook of Birds of Eastern North America*, Rev. ed., 1916, 88-97; H. W. Bates, *The Naturalist on the River Amazons*, 1892, 13, 139; G. H. Thayer, *Concealing-Coloration in the Animal Kingdom*, 1909, 24 ff.

the alleged sexual apathy should have disappeared or have been transformed quickly into its very opposite with certain savages that have been only recently affected by civilization. Moreover, the alleged coquetry of savages observed in both sexes seems to be in contradiction with any alleged apathy of the male alone; and with both sexes apathetic the coquetry would seem to lack motivation.

There is also some discrepancy in the statements of Westermarck that, with women in a condition of comparative slavery and in the minority, both sexes indulge in coquetry. Such behavior would seem to belong more especially to a stage of development in which men are scarce and woman's position more independent. The suggestion that even under the conditions of savage life woman may choose her lover, if not her husband, seems to be based on the supposition that personal beauty other than that founded on physical strength is the foundation for the alleged choice, whereas it is fair to assume that in primitive life physical strength and dexterity, quite apart from all adornment that is not the symbol of such strength, is the primary motivation. The elimination of the unfit males would in that case be quite similar to the elimination of the unfit males among the lower animals. The further suggestion that under the given conditions of inferiority savage men are not indifferent to the feelings of their mates implies a degree of refinement and susceptibility probably far above the level of the savage. Moreover, the use of clothing by harlots and in the so-called erotic dances might be for the very purpose of preventing sexual excitement, aroused in the latter case by the intimacy of the dance. It is difficult to imagine savages sexually so apathetic that they are obliged to engage in special dances to arouse or develop a desire which seems, indeed, to be the tacit presupposition of the dances in question, although it is a fact that the savage's lack of initiative in certain other undertakings is compensated for by dances which incite him to activity.[23]

With nudity the norm, it is plain that the novelty of clothing would serve to attract the attention; but would it imply anything more than this, so that by means of it a sexually unattractive savage might become more *attractive*? Even granting that it might, it would seem on the same basis that, with the general adoption of clothing, a return to absolute nudity would soon be indicated, unless this were already objectionable on other grounds. After clothing had once been in general use, it is easy to see how coquetry might come to be secondarily effective through the function of some clothing as ornamentation.

[23] Cf. Hirn, *op. cit.*, 247 ff.

In any case the theory interprets ornamentation in a manner that ignores the function of true ornamentation as developed by Lotze, Lipps, and Selenka. Even though we might come to derive the first use of clothing from the impulse to beautify or adorn the body, it does not seem necessary to think of such adornment exclusively or primarily in terms of sex stimulant, unless we are committed to something like a Freudian platform. An interpretation of ornamental clothing in aesthetic terms would be quite as possible for the savage as for the civilized children who decorate themselves spontaneously in the various ways described by Lotze. In the case of the male savage, even neglecting the subjective function of ornament emphasized by Lotze, self-decoration might be quite as well for the approval of his equal and superior male companions as for the inferior females; although it might prove to be also, secondarily, a sex stimulant for these. Probably, however, more practical motives are primarily operative in the selection of mates in savage life.

It must not, however, be overlooked that a great deal of the scarification, tattooing, and other decoration can be interpreted as being 'ornamental,' only by doing such violence to the meaning of the term as occurs when Grosse suggests that while these things are not ornamental for us they may be so for the savage; and something similar might perhaps be said even with respect to the interpretation as symbolical clothing of certain articles worn. In many cases the only term that seems to be applicable with strict justice to the facts as they lie before us is the rather general designation of differentiation or 'distinction.' Scarification, tattooing, mutilations, and all articles worn serve at least to *distinguish* the wearer or possessor and thus to enhance his self-feeling in contrast with beings not possessing such marks, whether or not they function specifically as clothing or 'ornament,' whether or not they have magical or religious significance, and whether or not they are signs of especial prowess. The lack of consistency in their alleged specific uses might thus in a way be accounted for. Probably the psychic life of the savage is far more complex or less explicitly differentiated than we in our zeal for simplification are wont to regard it, so that in the assumption of clothing many motives were doubtless in concomitant operation.

One such motive to the initial assumption of clothing is suggested by the fact already mentioned that savages knock out, file, blacken, and brown their teeth, on the testimony of many investigators, *in order to distinguish themselves as sharply as possible from certain lower animals*; they even make fun of the teeth of Europeans, saying that the mouths of the latter remind them of dogs, horses, etc., and for this reason they find them sexually unattractive. Is it not possible, then, that they

also paint and dress the hair primarily because of a similar impulse to distinguish themselves from certain lower animals, which they despise or regard in any case as their inferiors, rather than from any specific aesthetic impulse to adorn themselves? They would alter the natural condition of the hair, not for the purpose of any equivocal 'ornamentation,' but merely for the sake of distinguishing or differentiating themselves from their inferiors. Similarly with respect to the pubic and axillary hair. The fact that the pudenda are frequently tattooed in a manner that preludes or simulates clothing, would suggest that here, too, as in the case of other parts of the body, painting had preceded tattooing, which would in the sequel perhaps lead to clothing proper. Moreover, all these distinctions, once they had been established, might come to function in various other ways, just as the coloring and deformation of the teeth (possibly also of the head, feet, and other parts of the body) come, with the races practicing such customs, to be indispensable sex stimulants, since they become social norms for the groups involved.

In a paper read before the Southern Society of Philosophy and Psychology in 1917 on "The Significance of Beauty," Professor Dunlap referred to an aversion to the exposure of the axillary hair by women to-day and might also have called attention to the allied practices of shaving, the tonsure of the clergy in the middle ages and at the time of Leonardo da Vinci, to the fashion of removing the eye-brows, which seems to have returned of its own accord only recently. Dunlap suggests that the aversion to exposure of the axillary hair (which would be explained today, I suppose, on the ground of its suggestions of animality) may be a survival of the practice among Greek women of removing the pubic hair that has set an aesthetic standard. His belief that this practice of the Greeks was due to a desire to be rid of pediculi may indeed indicate a partial motivation for the custom, which would, however, instead of beginning there, be operative all the way from savagery to civilization; but why may it not be regarded quite as well as the survival of an older aversion to hair of the body seen among many savages, so that the explanation given for the aversion to-day would be the natural and universal one throughout the ages? The basis for it might well be found in a general tendency to disparage or vilify whatever the individual or the group does not itself possess. In many cases savages seem to manifest such an aversion by plucking out the hairs of the beard and of the eye-brows,[24] and they sometimes, as we have seen, regard short hair as a symbol of chastity. In other cases they alter the natural condition of the hair of the head by elaborate manipulation. Such an inferred primitive aversion to hair is indeed presupposed by Darwin in his theory that hairlessness in the human race is to be accounted for by sexual selection.[25]

[24]Heinrich Wölfflin, *Die klassische Kunst*, 31, explains the absence of eyebrows in the Mona Lisa (which was restored in copies of a subsequent period) by a reference to Cortigiano, who states that the removal of the eye-brows was a prevailing fashion with the women of the period which disappeared soon after. In a legend of the Cray Fish clan of the Choctaws mention is made of the fact that the Cray Fish had the hair plucked from their bodies before they could become men, cf. J. G. Fraser, Totemism, *Encyclopaedia Brittanica*, 9th ed., 23, 1911, 467; also James Mooney, *Myths of the Cherokee*, 325 (cited by Wundt, *Völkerpsychologie*, Bd. 2, 153.).

[25]Westermarck, *op. cit.*, 2, 20 f.

Whether in lieu of any evidence for the existence of a hairy race of men we accept his account of the matter, or whether, on the assumption that human life began in the tropics, we may infer that the race has developed under the influence of cooler climates more hair than it had originally, in any case it seems clear that man in a condition of comparative hairlessness might well desire to remove from observation or alter in various ways any such characteristic as hair which connects him with beings necessarily regarded by him as his inferiors in many important respects; and this impulse would naturally be stronger in proportion as the group to which the individual belonged was lacking in hair.[26]

Something like this seems easier to believe than the thought that primitive man is naturally ashamed of his naked body, his genitals, or even the sex phenomena to which St. Augustine refers.[27] Indeed we are informed on the contrary that savages are often very proud of their pudenda; so that in this sense *naturalia non sunt turpia*. On the other hand, the principle is manifestly restricted by the savage's treatment of his teeth and by other mutilations and alterations of his *naturalia*. It would seem that, not in themselves, but only when all these things suggest inferiority or identification in some way with a being of lower status does primitive man perhaps regard them differently. For primitive man in his rudest stage of existence (in which "though destitute of almost everything which we regard as the necessities of life" he still possesses 'ornaments') the only inferior being is the lower animal; so that the same impulse that leads to rivalry with his equals, to the attempt to exalt himself at the expense of his inferiors, and furnishes the general psychic foundation for classes in savage and civilized life might lead primitive man to attempts to distinguish himself from lower animals in certain ways which would doubtless be especially pronounced in the case of animals like the anthropoids that resemble him most closely. The sense of shame—the general negative aspect of the impulse in question—might be largely reducible to a fear of limitation or relative annihilation of the feeling of self through degradation to the status of a being whom the individual for various reasons regards as beneath him.[28] Shame for the naked body, together with the mutilation and scorn of the flesh which has played such an important rôle in civilization, might, then, in a measure go back to some such primitive situation. Sexual modesty proper might be due in part to the aversion to being reduced to the status of the animal through the natural appearance of the pudenda, in part to the fear of being reduced to the status of the rejected lover;[29] and clothing might then come to be used partly for the purpose of concealing premature expressions of sexual emotion, which in themselves, apart from the associations indicated, could hardly be the cause for shame St. Augustine supposed them to be. The privacy and secrecy of sex relations with some savages—although there are others who are said to copulate as publicly and unconcernedly as the lower animals—may be explained perhaps partly by this impulse, partly from certain aesthetic motives.[30]

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[26]The artistic treatment of the beard by the Assyrians, to which Wundt refers, might depend similarly on their being a comparatively hairy race.

[27]De civitate Dei, XIV, 17 (Migne, Patrologiae cursus, XLI, 425 ff.); cf. also Westermarck, *op. cit.*, I, 539 f.

[28]In this connection it is suggestive that Latin *pavor* and *pudor* are both derived from he same root *pu*; the original meaning of which is "beat, cut, or strike." Cf. Alois Vanicek, *Griechisch-lateinisches etymologisches Wörterbuch*, 1877, 539.

[29]Anthropologists relate that the shame at being rejected is among some savages so great that it results in suicide.

[30]Such are suggested by Weininger, *Geschlecht und Charakter*, 1905, chap., "Erotik und Aesthetik," 341 ff.

The chief objection to such suggestions is found, perhaps, in the fact that some savages tattoo themselves with animal forms, arrange their hair to represent the horns of animals like the deer, and otherwise endeavor to assimilate themselves to their own totemic animals. To what extent this is a kind of play or dramatization is perhaps debatable. The fact, however, that savages may admire or actually treat as equals certain animals, notably birds, beasts of prey, and the animals upon which they depend for their sustenance, does not preclude the inference that they doubtless despise very many others; and the circumstance that those who have totems worship these, while, on occasion, they also kill and eat them, involves a contradiction for which there is no satisfactory explanation in the accounts given either by the savages themselves or by their interpreters The natural inference would seem to be that while primitive man might admire or reverence a given animal for its courage, cunning and certain other qualities, he would nevertheless consider a being that could be circumvented and killed by him as essentially inferior, even in the case of his own totem. Certainly, in the later periods of savage mythology, the transformation into an animal form by means of witchcraft is regarded as a punishment and degradation. In any case totemism and the mythology of some savages is probably a late development and it is by no means a universal phenomenon of primitive culture; like all religious and social systems, totemism doubtless stands for a rationalization of customs that have long been in operation for other causes. Marks of distinction, such as scarification, painting, tattooing, wearing of trophies, badges, and the like, may well begin with no ornamental value whatever, and then, under the aesthetic impulse and with developed technique, develop later into true ornamentation that should acquire religious significance through its antiquity, and moral value through its social effects. Finally, while in the discussion of Lotze explicit provision is made for the enhancement of self-feeling only through form, the suggestion in question would provide also for the subjective and objective effects of color as well.

Whatever view of clothing we may feel inclined to accept for the savage, it is plain that the significance of clothing and adornment to-day, in cold climates and with Northern races, is different from what it is in Africa. Clothing for the modern man aims to be practical and for the male has largely ceased to be aesthetic. The body of the industrial male is encased in mathematically straight lines and *pour surcroît de malheur* the trousers are creased. Instead of revealing, as did the Greek costumes, what has been called the most beautiful object in all

nature, clothing may function to conceal defective bodies produced by the emphasis on mental and specialized physical efficiency to the neglect of physical beauty and perhaps of physical well-being.

The clothing of modern woman has usually been more adorned than that of the modern male, if not always much more aesthetic, both with respect to color and to form; but hand in hand with the development of feminism and the industrial rivalry of the sexes, there has been a change in the direction of a smarter and more business-like garb. This was doubtless accelerated greatly by the late war, partly as a result of the general practical military atmosphere, partly, I suppose, directly as a result of costumes worn by women in camps.

I doubt, however, whether anybody who regards the matter without prejudice will claim that the newer styles represent a descent from a higher plane of morality, or will attempt to make them responsible for any alleged decline in sex morality, any more than he will credit the prophecy that bobbed hair will result in bald-headed women with beards. Without wishing to be identified with any of the European cults mentioned at the beginning of this paper it seems to me personally that the new styles represent in many cases an advance aesthetically that should make for a sounder morality and possibly in some small way for racial improvement. It is of course possible that recent dress may have been responsible for some laxity on the part of the generation used to other styles; but it is hard to believe that any such condition alleged to be present in the younger generation can be ascribed to this. Within limits habit and custom would be the determining factors in matters of this kind.

Granted the fact of laxity, it can be more readily explained as a phenomenon due to the emotional orgies and the informal hurried marriages and matings of the recent war-period, as well as to the scarcity of men following upon war. This last phenomenon has been frequently observed in the past, the period subsequent to the Thirty Years' War having been especially notorious in this respect. There are also many analogous instances in the history of savage tribes, whenever through war, pestilence or other causes the number of men has been so reduced that women are greatly in the majority. Such a condition, if present, will hardly be corrected by the promulgation of edicts concerning the styles.

The fact of woman's recent emancipation, whether for good or ill, is clearly symbolized in her present attire, which permits her greater freedom of bodily movement than ever before; and when the high heels shall have followed the train, hoop-skirt, long skirt, and corset nothing of real value will have been lost. This is also true with respect to the primitive custom

of painting the face. On the other hand, it is certainly desirable that the impulse of woman to adorn her body in legitimate ways shall persist. If we agree with Schiller and Wundt with respect to the intimate relationship between aesthetics and ethics, and especially with the view of Schiller and Selenka that such ornamentation of the body is a case of legitimate *moralischer Schein*, we may well hope that this tendency shall be kept alive and that eventually some reflex influence of it may become manifest in a more aesthetic garb for man.

THE PSYCHOLOGICAL APPROACH TO ESTHETICS

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The desire to account for the facts of esthetics in terms of a prevailing psychology has been manifest throughout the recent history of the subject. Such accounts have sometimes lapsed into obscurity with the decline of the general view in question, and have sometimes made definite additions to the stock of ideas on esthetic matters. Today, however, if the dictum be true that there is no longer psychology, but only psychologies, this procedure is less applicable than when a more uniform attitude could be adopted. We must first catch our psychology before we can apply it; we must select from a large body of claimants to the title, and be exposed to criticism from the holders of the positions we reject. It therefore seems a fair question whether something may not be gained by reversing the process—that is, by seeing what sort of psychological position is suggested by, or would follow from, the more or less admitted facts of esthetics.[1] The present paper will make that attempt by first summarizing some previous contributions that seem adapted to the purpose, and then outlining a method by which their results may, with some additions, be applied to the treatment of esthetic problems.

I

It seems no rash assumption to hold that the main problems we have to deal with fall within the general field of problems of value; we must therefore first ask what occurs in a consciousness which takes account of a value. It seems to me clear that perception of value depends primarily on a certain detachment from the current of experience on the part of the percipient. Anything which is regarded simply as a signal or a cue for action can scarcely be considered as in itself valuable, though of course it may have value as an element of the total situation. The point is that it can not then and there be felt as valuable unless the percipient can in a sense 'stand off' far enough to see it as an element qualitatively distinct. Obviously, there are many occasions in ordinary life when we have neither time nor

[1]For my general position on these matters see, The scale of esthetic values, *J. Philos.* 21, 1924, 617-631. The three chief determinants of esthetic value are there defined as interest in sensuously agreeable material, interest in the complete of its kind, and interest in a 'good job' at meeting the requirements of a situation (621).

attention to devote to such aspects, and when, consequently, values are largely or entirely unfelt. But a consciousness of a purely automatic kind would hardly be what we mean by a normal human consciousness; and consequently we also find situations in which values of one sort or another definitely emerge.

The resulting constitution of consciousness has been most clearly defined, by MacDougall,[2] as a system of habits and a system of ideas, which are progressively organized and elaborated. "Each of these factors," he says, "has a specific function and a continuous history in mental development. The system of habits represents the organism's response to the permanent aspect of the environment; the system of ideas represents its response to the variational aspect. Wherever the system of stimulations presents recurrence, the organism responds with a habit; wherever that system presents novelty, the organism responds with an idea."[3] Personal experience is thus more or less stabilized between these two levels. "Without habit experience would be an irrational choas; without ideas it would have no existence—for experience is not posited of any organism in virtue of adaptation in general, but only in so far as adaptation constitutes an ideal process determined by a specific aim and a sense of value."[4] Its simplest form is "attention to the stimulus. Instead of provoking an habitual response, it arouses an awareness of its own quality and of the relations in which it stands."[5]

Whether or not this account holds for all perception of value—and for my own part I believe that it does—its fitness for the discussion of esthetic value is unmistakable. A purely routine, habitual type of mind would surely be unconscious of such values; and we find in actual experience minds so nearly of that type as to be practically insensible of them. At the same time, habit has its place both as affording a basis or background with which the value-perception may be contrasted, and as furnishing, by its easy and natural functioning, a pleasurable element of considerable importance in the total esthetic situation. But this is obviously not a complete account. The interaction of the two levels, habit and idea, creates in the percipient a more or less definite state of tension. It is possible both to conceive, and to discover in actual experience, many degrees of this tension, from a degree so slight as to be barely perceptible above the level of habit to one so intense as to border on self-forgetfulness or mystic rapture. Moreover, the individual percipient may exhibit many degrees of this tension

[2]Robert MacDougall, The system of habits and the system of ideas, *Psychol. Rev.*, 18, 1911, 324-335.

[3]*Op. cit.*, 327. [4]*Ibid.*, 335. [5]*Ibid.*, 326.

in his own experience, according to his state of adjustment in any particular set of circumstances, and according to the nature of the particular situation offered to him for esthetic response.

This notion of tension has played a somewhat indefinite part in psychology for a considerable time. Some twenty years ago Bawden discussed it in relation to the 'functional' psychology, and defined it as the emergence of consciousness in a disturbed state of equilibrium. "The organism, with the brain as its neural center of gravity, is a machine made up of a delicate system of balances. When this equilibrium is disturbed beyond a certain point, which varies according to the inheritance and previous history of the particular organism, consciousness emerges. Consciousness represents what, comparatively, we may call the tensional equilibrium of the organism, whereas habit represents its relatively stable equilibrium. This conception is divested of all objections from the metaphysical side, if it be constantly borne in mind that consciousness is no more an entity than habit, and that, like habit, it simply represents the life of the organism under a given set of conditions."[6] Again, "What are the conditions of consciousness? What are the laws which determine when an act becomes conscious or ceases to be conscious? These are: the law of tension or obstruction in activity, and the law of habit or facilitation in coördination. By the law of tension is meant simply this, that consciousness appears only when the process of action is relatively impeded or interrupted. Action is going on all the time, in tropism, reflex, and instinct. But these become consciously performed acts when there arises stress in adjustment, whether the forces of the tension be intra-organic or extra-organic."[7] Another writer who has used the notion is Janet, who has recently summarized its advantages as a means of bringing together a wide range of psychological phenomena.[8]

The fact that neither of the writers just mentioned gives us a very clear notion of the mechanism (if any) underlying this tension is no argument against its use as a descriptive concept. As Dumas himself remarks at the end of a chapter on the expression of emotions: "Est-il nécessaire, en terminant ce long chapitre, de constater que la plupart des problèmes qui y ont été posés ne sont pas, en l'état actuel de nos connaissances, susceptibles d'être résolus complètement, ou même d'être résolus du tout? Qu'il s'agisse des conditions psychologiques ou biologiques de la mimique, du mécanisme psychologique ou cérébral des émotions, de leurs centres, de leur nature, nous avons toujours abouti à des solutions incomplètes."[9] We are therefore not debarred from the use of the concept in discussing the esthetic experience, if we feel that we cannot conveniently do without it; and of the existence of tension as an empirical fact in the esthetic experience I entertain no doubt whatever. Moreover, it is a means of bringing together, and making coherent, various other views concerning esthetic data. For instance, it readily accommodates Edward Bullough's important doctrine of 'psychical distancing;' it enables us to accept empathy or *Einfühlung* as a useful principle without trying to extend it, as its more enthusiastic adherents incline to do, to every part of the esthetic field. Since, as we shall see in a moment, it adapts itself to a most important aspect of that field, no further justification is needed for regarding it as fundamental to any psychological account of the esthetic experience.

[6] H. H. Bawden, The functional theory of parallelism, *Philos. Rev.*, 12, 1903, 309 f.

[7] H. H. Bawden, The meaning of the psychical from the point of view of the functional psychology, *ibid.*, 13, 1904, 299.

[8] Pierre Janet, La tension psychologique et ses oscillations, in G. Dumas, *Traité de psychologie* I, 1923, 919-951.

[9] G. Dumas, *Traité de psychologie* I, 1923, 686.

We have, then, as a general description of a consciousness that takes account of values, an awareness of qualities and relations more or less stabilized between an upper and a lower level, and displaying a variable tension, the rise and fall of which may easily become rhythmic. The appropriateness of this for describing the esthetic consciousness has been shown by Bawden in another set of papers, devoted especially to aesthetic values.[10] He points out, for instance, that tension is also a balance between intellectual and emotional elements. "A state of absolute emotional saturation is never actually realized any more than a state of purely unemotional intellectual cognition. These represent limits within which experience moves; limits which if ever reached would mean the destruction of consciousness."[11] This at once introduces possibilities both of conflict and of increased richness into the esthetic experience. "In the case of esthetic emotion there is the maximum of stimulation compatible with a final reinforcement by the corresponding intellectual habits or apperceptive systems,"[12]—in other words, there must be a balance-wheel or control to stabilize the tension, in order that the value may be apprehended. Hence, "if imagery in general represents controlled impulsive and habitual responses, the esthetic image represents the maximum of such mediation or control compatible with the experience as a whole remaining pleasurable. And since all successful control is normally pleasurable, it follows that all practical and intellectual reconstruction of experience tends to culminate in an esthetic moment."[13] This notion of control is, as we shall shortly see, an important addition to our means of analysis.

This enlarged conception has been brought still closer to the most specific traits of the esthetic experience by Parkhurst.[14] In two papers published in 1920 she showed how the intermittent character of consciousness may be interpreted as a challenge to us to make the most of its possibilities, after a fashion to which the artist gives us a clue; and further, how the whole course of human evolution may be read as a constant striving toward mastery of the environment, taking the shape of actual control when it can, and of an aspiration toward control when thwarted by unfavorable conditions. This added notion of mastery is the key to certain traits which the simple notion of control does not fully explain, such as Bacon's famous statement of the use of poetry "to give some shadow of satisfaction to the mind of man in those points wherein the nature of things doth deny it." Moreover, she shows us how rhythm, present from the beginning in the living organism, favors maximum ease and facility of performance, but likewise induces unconsciousness, a lapse to the habitual, reflex level. Non-rhythmic activities, on the contrary, favor maximum awareness of response, but a minimum degree of rapidity and ease; the mere effort to acquire control may, for a time at least, submerge the perception of the value intended. Here, then, is yet a third aspect of tension, as something to be maintained between a rhythm not allowed to lapse into the uniformity that brings unconsciousness and a break in rhythm not so sharp as to destroy the underlying continuity. This inclusion of the rhythmic factor that plays so great and so obvious a part in all esthetic activity is a valuable addition to the availability of the concept of tension.

Putting together these various contributions, we have in the tension necessary to the perception of esthetic value a contrast of specific aware-

[10] H. H. Bawden, The nature of aesthetic value, *Psychol. Rev.*, 15, 1908, 217-236; The nature of aesthetic emotion, *ibid.*, 265-291; Aesthetic imagery, *ibid.*, 16, 1909, 124-141.

[11] *Psychol. Rev.*, 15, 1908, 225.

[12] *Ibid.*, 268.

[13] *Psychol. Rev.*, 16, 1909, 131.

[14] H. H. Parkhurst, The evolution of mastery, *Int. J. Ethics*, 30, 1920, 404-422; The obsolescence of consciousness, *J. Philos.*, 17, 1920, 596-606.

ness with habit, of non-rhythmic with rhythmic activity, of intellectual and specific control with emotional and wished-for mastery. In view of the interaction of all these, it is easy to see why individual esthetic experiences should vary so widely as we actually find that they do; to see also that all genuinely esthetic perception is active, not passive, as Bosanquet, in opposition to the 'associationist' psychology, made clear some thirty years ago.[15] Hence we need not be surprised to find that unexpected shifts which are yet felt to be parts of a single experience should accompany many of the higher reaches of esthetic reaction, as Catullus realized when he found that he could hate and love in the same moment. To put the matter in a form which is only outwardly paradoxical, tension allows us to 'enter into' the esthetic experience as conscious participants at the same time that we 'stand outside' it lest it or we be absorbed and the value, in consequence, dissipated.

But the notion we have thus built up is too complex to be immediately applied to the analysis of specific esthetic situations; we need a device by which we can, as it were, 'spread out' or magnify the situation, in order to see its constitution in any specific instance. This device I find in the conception of the esthetic situation as essentially triadic. By the help of this conception we are enabled to do justice to the various aspects of the esthetic experience without confusing them, and at the same time to prevent their falling apart or being set in misleading opposition to one another. It is further important because it occurs not only in a complete state but in states relatively less complete, and thus provides a means of approaching certain problems which prove refractory to other modes of treatment. Our next step, then, is to determine the nature of this esthetic triad.

If we construct an ideal scheme of the general esthetic situation, we find on the one side an experience in which its possessor has discovered an esthetic value; on the other, an experience into which that value is to be transmitted; and between the two, a device by which the transmission is to be effected—or, in simpler terms, an artist, a work of art, and a recipient. If we assume that the original value is both singly perceived and efficiently embodied, and then received without perturbation by irrelevant influences, we shall have the triad in its 'purest' state. But it is obvious that such a state is not always actually realized, and that even when it is, there may be unmistakable differences in the values revealed. The initial experience may be more or less dense or rich, and one artist may get more out of his experience than another can; while the sensitiveness and single-mindedness of recipients are notoriously variable, and there are also the individual traits of the work of art to be taken into account. From the strictly psychological standpoint, however, the first necessity is an accurate descrip-

[15] B. Bosanquet, On the nature of aesthetic emotion, *Mind*, N. S. 3, 1894, 153-166.

tion of the esthetic aspects of the experience of artist and recipient respectively.

It is also obvious that the complete esthetic triad does not often exist intact for any great length of time. An artist's period of activity may not be long, and must in any case be closed by his death; a recipient's period of acceptance may be equally interrupted. Both Shakespeare and the audiences for whom he wrote have long quitted this mortal sphere; and many living artists have doubtless not yet found their fit audience. But the work of art, by stabilizing the situation into which it is introduced, makes possible the constitution of new triads, and also serves as the basis for inference as to the traits of the original ones. In the case of Shakespeare, for instance, we have not only the plays that he wrote, but a body of information about the conditions amid which he wrote them, and also the chance of working backward from the plays to some conjectures about his own mentality. This means that the work of art is not and cannot be separated from experience, that it gets its significance precisely because of the experiences between which it is placed, though the generating experience may have passed away, and one receiving experience be replaced by another. Take the familiar remark made by the reader of a refractory novel, to the effect that he "can't get into it." He means that he has not so brought it close to his own experience that he can pass from that to the experience embodied in the novel; but there would be no point in the remark unless the work did embody an experience which could, under appropriate circumstances, be entered into.

The triads we actually encounter, then, are often temporarily incomplete; and two causes in particular render them so. One is the absence of recipients, or at least of properly qualified recipients; the other is the necessity, in certain forms of art, of a partial recreation of the work by others than the artist himself. It is notorious that many poets and musicians have had to wait years before they could count on an understanding public; and some works have not been recognized at their true value till long after their creators' deaths. Again, a building, a painting, a statue, can be seen if we can be where they are; but a piece of music must normally be audibly played, and a drama be acted. While these conditions are in part accidental, as in the collection of especially intelligent persons in particular places, or in the diffusion of skill on a particular instrument, they are not pure accidents, and their practical importance in the development of the several arts should never be overlooked. They do not, however, require further discussion at this point; and we may pass to an examination of a model triad supposed to display all three of its factors in a fully developed state.

II

The first impulse toward the constitution of such a triad must obviously lie in the perception, by the artist, of an esthetic value somewhere in his experience. It does not appear that we can profitably inquire what sort of experience is most likely to yield such a value; the records of artistic achievement show conclusively that artists have appeared in all sorts of material conditions, and at all points of the social scale. There are, to be sure, conditions that are more favorable than others to artistic development; but the cases in which apparently favorable circumstances fail to be improved, the even more obvious failure of means of artificial encouragement, and on the other side the numerous cases of successful emergence from adverse conditions, are enough to show that external encouragements and discouragements are at best merely contributory causes. Nor can we lay down with any greater exactness what qualifications an artist should have. The actually observable variety among artists stands in the way of reducing their attitudes to any standard patterns, least of all to the conventional notion of the artist as a casual, dreamy, impractical soul, hopelessly adrift in a practical world. Those who have best illustrated this notion are either not true artists at all, or artists of an inferior sort; those who seem to do so are usually perceived in the inevitable reactions from a labor not open to inspection, or in a more or less deliberate attempt to keep off the uncomprehending.

It is likewise impossible to reduce to uniformity the various motives which set artists to work and keep them at it. The mere pleasurableness of activity is no more generally effective with them than with men in other stations. If some artists work quietly and comfortably, others work like "mad laborers," as Chesterton aptly said of Carlyle. Balzac's secretary said he would not change places with his chief for the fame of Napoleon and Byron combined. There is no necessary connection between either attitude and artistic merit; it is merely a question of personal adjustment. Just so, a Milton may strive to make his own life a heroic poem, but not all poets follow his example, and he himself fell somewhat short of success in the effort. Even the desire to do as good a job as the circumstances permit, though it probably exercises a wider sway than the two motives just mentioned, is hardly universal. Those who aim at perfection most overtly are not always those who come closest to it, and happy accident sometimes carries off an unexpected triumph. It is, however, fair to say that such accidents cannot be counted on by the really lazy, and that the substitution of

short cuts and makeshifts for needed exertion is a prolific cause of artistic failure.

This last remark, I think, brings us as close to the artist's attitude toward his experience as we can profitably come. We seem justified in demanding of him that he shall view his experience for himself, and find there what he is genuinely impelled to find. But we cannot prescribe either the kind or the amount of what he should see. One man will get far more than another out of masses of experience which seem pretty nearly equivalent; one man exhausts his power of discovery, where another goes on constantly enlarging and strengthening his. But we cannot judge either success or failure in such discovery until the results are reported in communicable form. The man who successfully embodies and conveys a genuine esthetic value is in so far an artist, though he does it but once; a man who does it regularly is a greater artist; a man who only pretends to do it, or who deceives himself into thinking that he has done it, is not an artist at all.

It is obvious that this double demand for technical excellence and for sincerity does not leave the artist in unfettered freedom. Even if we grant him the largest possible liberty in contemplating his experience, he must still yield to the control of external factors when he tries to embody the results of his contemplation. He must work in a definite medium, the nature and capacities of which he must know. If he transgresses physical limitations, his work will simply collapse or be impossible; if he transgresses limits seemingly less tangible, such as the conditions of literary communication, he will none the less come to grief. Yet in all this he is suffering no more than the limitations of life prescribe for everyone. So far as man is finite, he must be limited; the practical question is, where?—and the artist is no more unfairly limited than anyone else. If he is sometimes more acutely conscious of the limitations, and able to protest against them more eloquently, that may make him more conspicuous, but does not really set him apart from his fellows, or make him a peculiarly notable victim of circumstances. I think it fair to say that the artists who "do protest too much" are in general not those whom we value most highly.

The upshot of all this is that the artist is not so radically different from humanity at large that we need a radically distinct psychology to explain his conduct and his productivity. A French writer once produced a "new esthetic based on the psychology of genius;" but I do not think that inspection showed either that the esthetic was new or that genius had a psychology of its own. In truth, the introspective testimony of an artist, even when it has been recorded—as in most cases it has not—is exposed to the defects of such testimony in gen-

eral, with the added ones imposed by greater excitability or ostentation, where those influences are present. Even so circumstantial an account as that which Poe gives of the genesis of the *Raven* can hardly be accepted unchecked. "When I read," says Bacon, "in Tacitus the actions of Nero or Claudius, with circumstances of times, inducements, and occasions, I find them not so strange; but when I read them in Suetonius gathered into titles and bundles and not in order of time, they seem more monstrous and incredible." Just so, when we consider the lives of Cellini or Whistler merely as sensational biographies, we find them strange enough; but after all, the record of their artistic achievement is really another matter, and when we approach them from the less excited cases with which the history of art abounds,they seem less strange even as human documents.

In short, the desire for a 'standard' type of artist is the result partly of the desire for false simplicity which we meet elsewhere in esthetic discussions, and partly of the desire that men who have accomplished striking results should themselves be striking. So long as the artist cannot readily be equated with his experience, and so long as what a man does may be inexplicable in terms of what he outwardly is, neither desire can be satisfied. But the longing for such an ideally satisfying type is keen, and so long as material to support it can be found, as of course it can, it will probably continue. But when we see, on the other side, how little we really know about many men who have actually achieved much, not to mention those of whom we know not even the names, we can hardly hope that the investigation of the artist himself will afford the best clue to the meaning of the esthetic. What we can hope to discover in this field I shall try to suggest later; meanwhile, let us accept the artist as simply a remarkable development and combination of sundry normal traits, and pass on to view our triad from the side of the recipient.

The psychological mode of approach is made easier in this case by the fact that, whereas most of us can enter but imperfectly into the artist's state of mind, we must all occupy the position of recipients if we are to perceive esthetic values at all. I do not wish to exaggerate the contrast, since I believe that the artist has no feelings which are absolutely peculiar to him; but the position of recipient certainly comes to us more frequently, and we can study it in its repeated occurrences with a much greater degree of ease and fullness. It deserves study too, since it is as necessary to the maintenance of the triad as is, on its side, the artist's activity. It is sufficiently evident that if there could be no works unless there were artists to create them, there would not long be artists if there were no recipients for their works. Mere desire for self-expression would not, I think,

be a permanently effective motive; at least the possibility of acceptance and encouragement seems to be presupposed, even if an artist may, in a given case, fail of its realization. We may therefore take the two sides as necessary complements, as indeed they must be, if the triadic view is sound.

What, then, are the qualities which the recipient must bring to the sustaining of the triadic situation? They may be defined as readiness to comply with the artist's legitimate demands, and freshness in the power to respond. Compliance must not be carried so far as to accept whatever the artist offers as if his mere offering guaranteed its value; that is the road to the acceptance of all sorts of fashions and futilities. But, on the other side, the recipient should not set up irrelevant considerations of his own, and challenge the artist to overcome them; that is the road to dogmatic imposition of standards, and to critical absurdity. A state of mere compliance becomes habitual, and so non-esthetic; certain marks are held to show that the artist has done his duty, but there is no tension, and hence no genuine response. But to urge that the artist must comply with *our* standards may be equally habitual, and equally destructive of response. We may accept Pater's dictum that "failure is to form habits," if we interpret it to mean that it is forming the wrong kind of habits; an interpretation which Pater himself admits when he defines scholarship as "habitual and summary recognition." Without some basis of habit, as we have seen, the tensional reaction is impossible; but the wrong kind of habit means at best the wrong kind of tension, at worst no tension at all.

Any form of habit, then, which deadens or impedes the fresh exercise of response is esthetically mischievous. The tendency of the merely pleasurable to pass into a vague state of satisfaction with habitual stimuli has always been a difficulty, and is an added proof of the inadequacy of mere pleasure as an esthetic determinant. Still worse is the lethargy which detests anything new 'on principle,' or which declines to make the least effort toward a genuine response. That "weakness of the spectators" which Aristotle noted in the audiences of Athens has returned to plague dramatists ever since, coupled as it often is with a reaction which drives them to an excess of emotional sentimentalism, equally to the upsetting of true tragic effect. The result of the latter impulse is a tension artificially excited and maintained, a parody of the genuine sort of tension, damaging both to the recipients who submit to it and to the artists who cater to it. If such damage is most obvious in the case of the drama, its ravages are not confined to that field, nor are they less harmful for being less concentrated.

Presumably, therefore, the type of habit-formation which favors the genuine esthetic tension is one which has learned to recognize the general properties and capacities of the artistic medium in question, so as to recognize that competent handling of it which is one of the esthetic determinants, and thereby to estimate the new esthetic values presented by the individual artist. This assumes, in the recipient, a sensitiveness which can be trained by use, but which seemingly cannot be artificially supplied if it is not there to start with. Now since such a sensitiveness can neither be uniform over the entire range of the several arts, nor uniform in the temporal experience of the individual recipient, variations are bound to arise, and we need not be surprised to find that esthetic judgments fluctuate. Even if the impediments caused by unsatisfactory habitual reactions could be wholly removed—as we know they cannot—there would still remain the difficulty that the individual cannot always maintain his responsiveness at the highest pitch. We have all, I suppose, had the experience of looking forward to a concert or an art exhibition from which we promised ourselves great satisfaction, only to find the actual experience disappointing; whereas some apparently accidental or trivial occasion has given us keen esthetic pleasure, because we were precisely 'keyed up' to it. Much that is called capricious in esthetic valuation is really due to conditions beyond the recipient's control; all he can do is to discount them, and to hope for better luck next time.

In view of all these facts, we must hold that the formation of the esthetic experience in any individual recipient is a process bound to be attended by difficulties. He must always be partially limited by the situations which he actually finds in his environment; he may not be able to react with equal sensitiveness to all those which he does find; he may not be able to maintain his sensitiveness at the proper pitch as often as he would like. But at least he can discern the directions in which the chief difficulties lie, and can do his best to remove them, by seeking fresh opportunities, by getting all he can out of those he has, and by endeavoring to keep his powers of response both fresh and disposable. It is obvious that different recipients will be able to do this in different measures, and neither we nor they should quarrel with the results, so long as they are sincerely reached. The one sure means of killing true esthetic activity is to pretend that it is there when it is not; honest inability to perceive a certain value may yield to further experience, but insincerity taints the entire process, and inevitably ruins it if allowed to run for long.

We can now see more clearly that the simplified and ideal triadic scheme with which we began is subject to many modifi-

cations in the actual examples. In a sense, the artist has done his part to establish it when he has embodied his chosen esthetic value; but if we consider in sequence several works by the same artist, our judgment of each is somewhat colored by the rest. Again, the only point at which we can estimate the artist's command of his means of expression is precisely in the work which he accomplishes; and to estimate that correctly, we must fall back on a knowledge of the medium gained from observing other instances of its use. Finally, as we have just seen, the same recipient may react differently to the same work on different occasions, and in the actual process of reception one recipient is constantly being replaced by another. Consequently, what we actually find is not so much a single determinate triad as a series of triads, fluctuating according to all the possible variations of tension in all the possible recipients. The work of art tends to stabilize these by focusing the esthetic value; but it focuses with varying sharpness according to the sensitiveness of the experience to which the value is transferred, and to the presence or absence of disturbing influences. It is therefore apparent that any over-rigidity in the triad taken as a mere concept is made up for in actual experience by the inherent variety in the process of reception, a variety for which the fluctuation in individual judgments is not too high a price to pay.

III

If the foregoing descriptive account of the psychological aspects of the triad be accepted as correct, what can we say of the means at our disposal for the more detailed study of them? In the case of the artist, for reasons already suggested, I believe that we cannot go very much farther. Any cases of direct statements by artists about their procedures of course deserve careful collection and scrutiny; but they are at best less numerous than we could wish, they do not always bear on the points which most need elucidation, and they must always be examined for unintentional misstatements. An artist is not always able to record the different stages in the working out of a complex conception in the exact order of their actual occurrence; and he may not have paused to note, or may be incapable of noting, the deeper forces which led him to do as he did. Consequently, the amount of good first-hand testimony on such matters is, and always must be, severely limited.

It would seem to be the part of scientific caution to be content with this limitation, and not to try to go beyond it by the alluring but specious device of interpreting fragmentary data—or, still worse, practically inventing data—in the light of a prevailing psychological notion. Many critics have tried

to work back from the existent facts of a work of art to the presumed state of the artist's mind in a way which is at best highly conjectural; and it has yet to be proved that the procedures of psychoanalysis are really any sounder. Even apparently objective data may be manipulated in a hazardous way, as is seen, for instance, in certain recent studies of Shakespearian punctuation,[16] or in the efforts to assign anonymous paintings and statues to definite artists by the sole help of technical criteria. When employed with care, and in conjunction with all the other external evidence available, such modes of study have their use; but when employed for their own sake, they are fatally likely to run into extravagance, and the only 'psychology' on which they are likely to enlighten us is that of the critic.

The other great resource of present day psychology, experimental procedure, seems equally inapplicable to the artist's mentality. We can scarcely subject the artist to laboratory conditions while he is in the actual course of production; and even if we could, it is to be feared that the resulting facts would not be of a nature to throw real light on his achievement. Since such procedure is obviously out of the question in the case of artists no longer living, a vast range of esthetic phenomena is by definition beyond our grasp, except as we can more or less reconstruct it. I repeat that I see no valid objection to such attempted reconstruction, provided that we recognize the extremely hazardous nature of the task, and the extremely provisional character of the results. What we are likely to get by it is not a contribution to scientific esthetics, but an experiment in hypothetical biography, the subject of which happens to be an artist.' The results may be interesting enough in their way, but we must beware of taking them for what they are not, and cannot be.

When we turn from artist to recipient, we seem at once to be on surer ground. We can all, as we noted above, be recipients; and there is no reason why the conditions of reception should not be made the object of experimental study. This, indeed, is precisely the nature of experimental esthetics as it has thus far been carried on. Here too, however, the results seem to be limited. The matters most susceptible of experimental treatment must be relatively simple and easily repeatable; which amounts to saying that they are for the most part rather unimportant elements in any actual esthetic situation. By themselves they do not carry us very far; and as elements in a more complex situation they may be obscured or altered. Hence the actual results of experimental esthetics have been somewhat

[16] Excellently summarized and critised by R. M. Alden, The punctuation of Shakespeare's printers, *Publ. Mod. Lang. Assn.*, 39, 1924, 557-580.

meagre, and even within their own limits they have not been free from uncertainty.[17] Moreover, they have sometimes been worked out with too small a body of subjects, making the extension of the results insecure; or, if a wider range of subjects is sought, the difficulty of securing uniform conditions, and of excluding disturbing factors, becomes steadily greater. It therefore seems unlikely that the direct experimental method can do more than determine the general conditions favoring the acceptance of the simpler esthetic values, discounting personal variations except in so far as they throw light on differences of individual sensitiveness, and so may help to constitute distinct types of esthetic reaction.

It may be thought that we are thus brought to a dilemma. If psychology be interpreted in a narrow and strictly experimental sense, and if everything not susceptible of experimental treatment be ruled out, it would seem as if the psychological aspect of esthetics must be severely limited. If, on the other hand, we use less rigid methods, like reports of personal experiences or widely distributed questionaires, we may be accused of forsaking proper scientific methods, and sacrificing all hope of certainty. It does not seem to me, however, that our condition is really quite so desperate. Until there is greater agreement among psychologists as to the exact scope and procedure of their science, it seems permissible to argue that both methods are legitimate, and that the second, under proper restrictions, may be used to supplement the first. If once we grant that the experiences of artist and of recipient are susceptible of psychological treatment—and I see no reason why we should not grant it—the question becomes simply one of the best way to accomplish such description, and if one device does not work, we are at liberty to try another, provided we do so with proper caution. A frank recognition of the difficulties in the road is not a reason for giving up, but only for being more careful; and if the esthetic situation is really as complex as it appears to be, there is the more reason for supposing that only a correspondingly elaborate attack on its problems can hope for success.

Such a hope, however, seems to me to depend on utilizing the notion of the triad as a means of keeping both psychological aspects of the situation always in view, even if, in default of evidence or in pursuit of a special aim, we temporarily emphasize one at the expense of the other. "The oral discussion," says Hoernlé in his report of the symposium on esthetic problems

[17]The work of Fechner and his school is reviewed by Charles Lalo, *L'esthétique experimentale contemporaine*, 1908. Some more recent contributions are discussed by Edward Bullough, Recent work in experimental aesthetics, *Brit. J. Psychol.*, 12, 1921, 76-99. For the present status of experimental procedure see Knight Dunlap, The experimental methods of psychology, *Ped. Sem.*, 32, 1925, 502-522.

at the Oxford Congress of Philosophy in 1920,[18] "turned chiefly on this very point: does the artist's creative activity or the spectator's enjoyment give the better clue to the esthetic psychosis?" Surely the truth is that neither by itself gives adequate clues, since neither in the long run stands by itself or functions by itself; and to concentrate attention exclusively on either aspect will at best over-simplify, and at worst disrupt, the entire situation. Furthermore, if two aspects of that situation are directly psychological, the third—the work of art—is not; and, as normally more permanent than the experiences between which it is interposed, it often serves as the most convenient point of departure from which to study the entire situation, and is our only basis for psychological inference when the situation is no longer accessible in its original entirety.

It may make the issue clearer if we briefly apply this notion of the triad to two specific problems of esthetic theory: the status of 'natural' beauty, and the possibility of self-criticism on the part of the artist. The existence of situations in which, by definition, no work of art is present, and which are yet felt to offer an esthetic value, has long been a disputed matter. The controversy on the subject has swung between the extremes of declaring only art to be beautiful, and of declaring that art is a mere parasite on nature; but it has gradually settled down to a recognition that nature cannot readily be so defined as to make possible an absolute separation of it from art. To say with Whistler that "art is art because it is not nature" is after all not to give a very clear idea of what nature is; to say with Hegel that works of art have a monopoly of beauty seems to fly in the face of ordinary experience. Yet if the triadic notion is sound, it ought to be possible to show that the natural situation can, without straining, be made to reveal at least its germ.

Let us take the simplest imaginable case, that of a natural situation which is felt not merely to be pleasurable, but to contain a positive esthetic value. Is it not true that we tend to note this latter as an aspect within the whole, an aspect which, if we had the requisite ability, we might very well try to embody in a work of art? Again, does not the photographer who seeks an effective result incline to wait for the appropriate background of clouds, or the appropriate lighting, which will make the scene look as it 'ought to look'? He does not take the situation as he finds it, but reacts to it in such a way as to emphasize a present value or to note the absence of the determinants of such a value; and he will recognize that he must either wait for more favorable conditions or accept something which he feels to be less

[18] R. F. A. Hoernlé, *Philos. Rev.*, 30, 1920, 67. The symposium, a landmark in the recent literature of the subject, is printed in *Brit. J. Psychol.*, 11, 1920, 1-56. See also the brief summary and comment by C. E. Whitmore, *Monist*, 31, 1921, 601-608.

valuable. In other words, every observer must partially detach the esthetic element from the total and consider it more or less by itself, if he is to perceive it as esthetic at all. The work of art would then simply carry this detachment one stage farther, and, by making permanent the value in question, supply the possibility of creating new situations after the first one has passed away or been passed out of. I agree that the complete triad as we at first formulated it is a development from the original natural situation; but I hold, also, that it is a continuous development.

The second question, that of the possibility of the artist's occupying the position of recipient toward his own work, has also been variously answered. The view which would regard the artist as having his own 'psychology of genius' is likely to deny it because of more or less mystical doctrines of 'inspiration,' according to which the artist mysteriously receives his impulse from without, and is the purely passive channel through which the work finds expression. No doubt evidence in support of this view exists; but on any showing it is not the whole view. There are numerous cases of artists who set about their work in a very business-like and objective way, and who are afterwards able to tell us a good deal about it. In terms of our tensional psychology, the esthetic impulse is valueless unless it is subjected to the retarding influence of a work of art which not only stabilizes it but allows it to set up new situations. But how far any individual artist is conscious of this process while it is going on, or cares to reconstruct it after it is over, will depend on his idiosyncrasy. An indication of this is the very different degree in which artists concern themselves about their works after they have been produced. Some keep systematic note of them and are much interested in their fortunes; others seem to care little about them, but at once go on to a fresh task. Such differences are natural enough, as soon as we have abandoned the notion of a standard 'artist-mentality;' and the sensible conclusion would seem to be that such self-division on the artist's part as shall enable him to see his own work objectively is entirely possible, but by no means universal.

If we now pause for a moment to review the various points which have thus far been made, I think it will be seen that we have in this notion of the esthetic situation as a tension between two levels, reconciling within itself a complexity of factors and differently adjusted in artist and in recipient, perhaps the only one which is adequate to the variations of the esthetic activity as we actually find it in experience. By its help we can find psychological equivalents for the intellectual element of control and the emotional element of mastery, and by the added device

of the triad we can keep both in contact with the work of art which is the vehicle of the esthetic value. Moreover, as Bawden previously saw, it helps to give us a right conception of the much disputed notion of 'freedom' in art. "If our preceding analysis of the relation of the sensuous and ideal elements in beauty is correct, it is not the presence or meaning of purpose, but the irrelevance of the purpose, which interferes with an object being beautiful. Meaning is no barrier to beauty if the meaning be intrinsic, *i.e.* relatively adequate as an embodiment of the relations which it suggests. We are free in the esthetic experience, but we are not free from all ends or meanings; we are free only from necessary relation to an extrinsic end. Indeed, the freedom is gained just by the controlled or relatively adequate organization of what would otherwise be transgredient ends, into terms of an harmonious but internally diversified system."[19] Again, "Not the presence of purpose but the particularity and arbitrariness of the purpose militate against beauty. There must be relevancy as well as elusiveness in order to call out those deep-lying intellectual habits whose exploitation along not too unfamiliar lines is pleasurable."[20] Art is in one sense not free, so far as it is bound to aim at the purest and intensest of embodied esthetic values; it is, in another sense, free to adopt all sorts of means towards that end. The freedom of mastery which ends in mere aspiration without corresponding control is barren; the bondage of control which issues in command of a particular expressive medium may be the artist's true means of liberation. It is the blend of mastery and control in the achievement of the esthetic value which reconciles the two, so far as such reconciliation is humanly possible.

What, in conclusion, are we to say as to the value of the psychological approach to esthetics? I believe that it can tell us much more about the psychological demands which are imposed upon both artist and recipients than about the causes of the esthetic experience. These demands, in a word, issue in the requirement of maintaining relevant sensibility at the highest possible pitch. The portions of an artist's experience or of his efficiency which he cannot bring to bear for artistic ends do not interest us esthetically; the ability of one recipient to contribute more in the way of response than another can do, to stand a higher degree of tension, determines for the former an intenser esthetic experience, not to be depreciated by another who is incapable of it. We must realize, that is, that genuine esthetic values are relative to the capacities of actual human beings, and that the important thing is to discover in the actual

[19] H. H. Bawden, *Psychol. Rev.*, 16, 1909, 132.
[20] *Ibid.*, 133

cases just what artists and recipients are capable of under the most favorable conditions, whatever those may prove to be. Any mode of psychological study which tends to reduce its results to a dead level of 'average' achievement is bound to leave out of account many of the most characteristic aspects of the esthetic experience. So long as the desire for 'objectivity' in psychology leads in that direction—as in certain cases it seems to do—I doubt if genuine esthetic theory will be greatly helped by it.

But it also seems to me unlikely that the psychological approach can ever bring us very close to the chief determinants of esthetic value; that what it tells us about the demands made on artist and on recipient can ever be translated into explanations of esthetic valuation. If we accept the triadic view of the situation, it is at once clear that at most only two-thirds of that situation can be interpreted in psychological terms; that the psychology of the individual artist is often undiscoverable; that the psychology of the recipient tends to reduce itself to a general statement of the elementary conditions that favor esthetic awareness; and finally, that neither of these sides is psychologically existent until an actual esthetic value has been discovered and embodied. Moreover, the control of such values by the sense of efficient technical handling falls outside the psychological field, and in fact demands a very different line of approach. If a large part of all this does not fall within the field of psychology, I should be at a loss what to name its location; but enough remains outside to explain why it is that esthetic theory can still be pursued while the rival systems of psychology are trying to decide which of them, if any, can swallow up all the rest.

AN EXPERIMENTAL STUDY OF FATIGUE IN THE AUDITORY MECHANISM[1]

By FRANK A. PATTIE, JR., Harvard University

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It seems that in the past most physiologists and psychologists have assumed that the auditory mechanism is not subject to fatigue. No mention is made of such fatigue in any textbook of physiology or psychology known to the writer except Nagel's *Handbuch*. The work done on fatigue in audition has been meagre, with technique not above criticism, and often with negative or uncertain results. In fact, the failure of previous investigators to demonstrate the phenomenon with any certainty led Professor L. T. Troland, relying mainly upon the negative results of Sewall, to say in 1914 that "stimulation of the ear with a sound of constant intensity does not result in a decrease in the apparent intensity of the tone or noise, once the tensor tympani is adjusted, if the attention is unwavering."[2]

The work here reported was undertaken for the purpose of clearing up the uncertainty connected with the problem of fatigue in audition. The problem was to find out (1) whether there is a decrease in the intensity of a tone after the ear has been stimulated for a definite period of time, and (2) whether this decrease, if it occurs at all, is conditioned by psychological (especially attentional) factors and is only an apparent decrease, or is actually due to fatigue of a physiological nature.[3]

[1]From the Psychological Laboratories of Harvard and Princeton Universities. The writer takes this opportunity to express his thanks to Professors H. S. Langfeld, E. G. Boring, and M. J. Zigler for suggestions about this problem, and to the Scientific Equipment Division of the Western Electric Co. for the loan of apparatus.

[2]L. T. Troland, Adaptation and the chemical theory of sensory response, this JOURNAL 25, 1914, 500.

[3]The meanings of 'physiological' and 'psychological' as here used are illustrated by the names given to Hering's and Helmholtz's theories of simultaneous visual contrast; the former is very often called a physiological, the latter a psychological theory. The words are not meant to have any definite theoretical (*e.g.* dualistic) implication.

History of the Problem

There have been three lines of research on auditory fatigue; investigations have been reported on (1) the effect of prolonged stimulation of one ear upon the intensity of tones subsequently heard by it, (2) the effect of such monotic stimulation upon the localization of a subsequent diotic tone, and (3) the ultimate disappearance from consciousness of very high tones near the upper limen for pitch. This paper is concerned with the first of these methods of investigation.

A summary of casual and experimental observations (except for the work of Huijsman) prior to 1905 is given by K. L. Schaefer in Nagel's *Handbuch*.[4] Albrecht[5] summarizes all the work prior to 1919. It is therefore necessary to mention here only the more important experiments.

J. J. Müller[6] in 1871 reported that a clang sounded weaker and less rich if the ear had previously been stimulated by the frequencies corresponding to the overtones of the clang. The clang heard in an ear thus fatigued was compared with the same clang when heard in the unfatigued ear.

Urbantschitsch[7] in 1881 used tuning fork tones conveyed to the ears through rubber tubes. A tuning fork, after sounding before one of the tubes for 10 or 15 sec., was partially damped; and after its tone had died out completely it was removed to the open end of the tube leading to the other ear, whereupon the tone was regularly heard for several seconds by the unfatigued ear. The fatigue reduced in this way the intensity of tones, but only of those tones having the same frequency as the fatiguing stimulus. The duration of the fatigue-effect ranged from 2 to 5 sec.

In 1884 A. Huijsman,[8] of the laboratory of F. C. Donders, reported experiments designed to test the conclusions of Müller and Urbantschitsch. The latter's experiment was repeated with the added precautions of keeping the *Os*' heads steady by means of a biting board and fastening the fork in a block. With these conditions, the results were negative with 5 *Os*; without these conditions the results were "uncertain." Urbantschitsch had said that a fork tone loud enough to be heard by both ears was heard as louder by the unfatigued ear, but the *Os* could not be sure that this was the case. Huijsman, after a long account of experiments on the disappearance of high tones, reports experiments which do show fatigue to some extent, but apparently the two sets of experiments are not brought into relationship with each other. He conducted tones of 240 v. and 250 v. of equal intensity successively to the same ear. After stimulation by one of the tones for one or more minutes, the tones were compared, but the intensities were "little or not at all different." The apparatus was then modified so as to reduce the intensity of these tones until they were barely audible, whereupon "the difference remained slight, and where a difference was noticed it was due to a loss in the tone which had been used as a fatiguing stimulus." Forks of 210 and 250 v. were used, but with less convincing results, because of the difficulty of judging intensity where there was such a difference in pitch. In another experiment a 250 v. fork tone was led by a branching tube to the ears. After 2 minutes' stimulation of the left ear, the tone was heard stronger in the right ear when the tone was pre-

[4] K. L. Schaefer, in W. Nagel, *Handbuch der Physiologie des Menschen*, 3, 1905, 509-512.

[5] H. Albrecht, Zur Frage der physiologischen Ermüdbarkeit des Hörnerven, *Beiträge zur Anatomie* [etc.] *des Ohres* [etc.], 13, 1919, 202-229.

[6] J. J. Müller, Ueber die Tonempfindungen, *Ber. d. sächs. Ges. d. Wiss., math.-phys. Kl.* 23, 1871, 115-124.

[7] V. Urbantschitsch, Zur Lehre von der Schallempfindung, *Arch. f. d. ges. Physiol.*, 24, 1881, 574-585.

[8] A. Huijsman, in *Onderzoekingen gedaan in het physiologisch laboratorium der Utrechtsche Hoogeschool*, 3de Reeks, 9, 1884, 87-142.

sented to the ears successively at low intensity. In several of these experiments no statement is made as to the number of *Os*.

The work of Sewall[9] was designed to give quantitative results. He planned to fatigue one ear with a given tone, then conduct the tone to both ears and have the *O* regulate the intensities so as to get equal loudness in the two ears. He hoped to use median localization in the head as a criterion of this intensive equality. The tones (from organ pipes and an electromagnetic siren) were conducted through telephones to the *Os*. The wires from the microphone near the source of sound ended in a primary coil, around which was a secondary coil, by adjustment of which the intensity of the tone received could be varied.

Preliminary tests showed that the adjustment of the sound intensities was very much modified, or at least rendered difficult, by the influence of tuggestion. In order to eliminate this factor, the apparatus was so arranged that the position of the regulatory coils gave no clue to the relative insensities in the two ears.

Sewall stimulated one ear, say the left, for one or more minutes, and then the circuit to the right ear was also closed; whereupon *O* had to say in which ear the tone was louder (judging this indirectly from the localization within the head). In the first experiments the *O* regularly had the impression, as soon as the circuit on the right was closed, that the sound was very much louder on that side and that the source of sound was located in the right ear. *E* then sought to diminish the intensity on the right side so that the sounds would appear equally intense after the fatiguing stimulation, but such an enormous attenuation was required for the compensation of the 'fatigue' that Sewall came to doubt the previous interpretation of the phenomenon. The localization, moreover, did not change gradually from the unfatigued ear back to the median plane, but suddenly. These facts led Sewall to believe that the phenomenon was due to an attentional factor; the sound in the unfatigued ear sets in as something new, while in the 'fatigued' ear the tone simply continues as before. To test this hypothesis as to an attentional factor, he interrupted the tone in the 'fatigued' ear before allowing the two sounds to act diotically. An interruption of less than a second was sufficient to cause the 'fatigue' effect to vanish.

Albrecht[10] in 1919 reported a small number of experiments with himself and Dr. K. L. Schaefer as *Os*. A tuning fork was struck (c^2, c^4, and c^5 were used), and the average time during which it was audible was noted. Then a whistle of the same pitch was used as a fatiguing stimulus for one or more minutes, and immediately thereafter the fork was again struck and the duration of audibility again noted. This period of audibility was regularly shorter than it was when the ears were unfatigued.

J. C. Flügel[11] investigated the effect of fatigue on intensity by the use of tuning forks and rubber conducting tubes after the manner of Urbantschitsch but with some improvement on the method. In a series of experiments on 5 *Os* 40 trials were made on each ear, and in 46 % of the trials fatigue was shown. There were no marked differences between *Os* or between ears of the same *O* with respect to the degree or duration of the fatigue, which seemed to reach a maximum after stimulation for 40 sec.; a 3 min. period gave no increase in fatigue. No difference was found between the fatigue-effects of a loud sound and of a weak sound. The duration of fatigue was seldom more than 30 sec. In an experiment designed to test

[9] E. Sewall, Beitrag zur Lehre von der Ermüdung des Gehörorganes, *Zsch. f. Sinnes., physiol.*, 42, 1907, 115-123.

[10] Albrecht, *op. cit.*

[11] J. C. Flügel, On local fatigue in the auditory system, *Brit. J. Psychol.*, 11, 1920, 105-134, esp. 123-133. The greater part of this paper is concerned with the shift of localization produced by fatigue.

the effect of a fatiguing stimulus upon the loudness of tones of other pitches, all combinations of the forks 256, 320, and 512 v. were used. In all 214 judgments were taken, and in only 10 % of the judgments did the *Os* report that stimulation by one frequency caused a decrease in the intensity of other frequencies. Flügel regards this apparent decrease as due to errors of observation, since in about the same percentage of cases the opposite effect occurred (*i.e.* the sound was heard longer on the fatigued than on the unfatigued side). Further experiments with two *Os* showed that transfer of the fatigue-effect did not occur outside the range of their DLs for pitch.

The latest work is by Weinberg and Allen.[12] A tone from a Stern variator enclosed in a sound-proof box was interrupted periodically by a revolving disc and the slowest rate of interruption was found which still gave the impression of a continuous tone. The ears were then stimulated by another variator-tone for two minutes, and another reading was taken. The tones ranged from 140 to 250 v. The results indicated that "as a result of fatigue the persistency of sound impressions is greatly increased. The increase is a maximum when the frequency of the fatiguing tone is the same as that upon which the measurements are taken." The entire range affected was about 8 v. on each side of the maximum, and only a few tones very near the maximum were affected to any great extent. An idea of the order of magnitude of the effect may be had from the statement that fatigue reduced the length of the separate impulses of sound necessary for the impression of a continuous tone from about 0.020 sec. to about 0.018 sec. A decrease in the fatigue effect as the fatiguing tone increased in intensity "was clearly indicated," as was also an increase of fatigue with an increase of frequency. The authors interpret their results in terms of the resonance theory of audition.

The thesis of E. Sch fer[13] lies somewhat out of the range of this paper because of the phenomena he studied. His work is rendered of doubtful scientific value because he does not say whether he used more than one *O* (presumably himself). He investigated the fact that, if one sounds two tones, one from a tuning fork and the other from a monochord loud enough to mask the fork tone, and then damps the monochord tone suddenly, an interval of 0.01-0.15 sec. ensues in which the fork tone is not heard. The louder the monochord tone, the longer the silent interval. After becoming audible, the tone reaches a maximum loudness which exceeds the loudness it would have if heard without the preceding monochord tone, *i.e.* the subjective intensity runs beyond the objective intensity, as it were. These phenomena are explained as "more a psychological than a physiological process," since they are interpreted as due to cortical processes correlated with a shift of attention; and Sch fer gives an elaborate psychological explanation, adhering very closely to Wundtian theory.

He also investigated the fact that the length of time a struck tuning fork takes to become inaudible is increased if a fork of not very different pitch is sounded at the same time. The accompanying fork is damped at the moment when, on the average, the first fork (sounding singly) becomes just inaudible, and it is then found that the first fork is heard a few seconds longer than this average time. Sch fer gave a physical explanation of this phenomenon; he believed it to be due to a slight shift of the locus of excitation of the basilar membrane caused by the interaction of the two frequencies. This shift, too small to be appreciated as a pitch change, would allow the fiber that would have been stimulated by one of the tones sounding singly to "rest" and therefore to respond to the tone so as to give a sensation for a few seconds after the damping of the accompanying tone. Hence Sch fer thought that this work was an indirect attack on the prob-

[12] M. Weinberg and F. Allen, The effect of aural fatigue upon the critical frequency of pulsation of tones, *Phil. Mag.*, 47 (6th ser.), 1924, 126.

[13] E. Schäfer, *Ueber Ermüdung des Ohres*, 1911.

lem of fatigue. He was not able to duplicate the results of Urbantschitsch with the latter's method, but by closing the ears before a tuning fork was struck and afterwards opening them at the time when the fork tone would otherwise be just inaudible, he regularly heard the fork a few seconds longer.

To sum up critically, it may be said that doubt is cast upon the efficacy of Urbantschitsch's method by the work of Huijsman and Schäfer. The only quantitative data on the method are given by Flügel, who demonstrated fatigue in only 46 % of his trials, a percentage which shows that fatigue, as revealed by this procedure, is a labile phenomenon. The disadvantages of this method are that it involves, in at least some of its forms, reports on the presence or absence of liminal tones and that these difficult reports may be influenced by such factors as suggestion, confusion by other sounds, diversion of attention, etc. Sewall found in his experiment that suggestion was a strong influence.

The reason why Sewall was not able to demonstrate fatigue seems to be the fact that his measure of fatigue was unreliable. He tried to have the *Os* judge on the intensities of two tones presented simultaneously, one to each ear. The judgment of intensity was hence indirect, mediated by the localization of the resulting (fused) tone within the head. The adjustments of the apparatus to secure median localization, when both ears were unfatigued, were subject to considerable variation; and Sewall says in this connection, "Man sieht, dass die Genauigkeit binauraler Tonstärkevergleichung sich hier als eine sehr geringe herausstellt."[14] Undoubtedly it required a greater intensity-difference between the two ears to cause an appreciable shift in the localization of the acoustic image than that difference which was produced by one of the ears having been stimulated for two minutes.

We have in the work of Schäfer no direct proof that the ear is subject to fatigue, since his interpretation of his results as fatigue rests upon a very shaky theoretical structure.

Some references to articles not mentioned in this paper on account of lack of space are given in a footnote.[15]

Apparatus

The instrument used to produce tones was an audio-frequency oscillator with a range extending from 440 to 4500 cycles per sec. The tones were produced in the telephones of a radio headset, and the intensity of the tone in one telephone could be varied independently of the other tone by passing the current through a variable resistance. A circuit diagram of the oscillator is given in Fig. 1.

Rubber holders or cushions of annular shape, about 5 cm. in diam. and 3.5 cm. in depth, enclosed the telephones and held them against the ears, making a snug and practically sound-proof joining around them. The diaphragms were about 1 cm. further from the meatus than they would have been if the holders had not been used.

A difficulty was encountered at the beginning of the experiments, in that the telephones, even when disconnected from the oscillator, would give a weak tone (through induction) when the oscillator was operating nearby. This trouble was removed by short-circuiting the wires leading to the telephones by means of small relays. When absolute silence was wanted in a telephone, it was short-circuited; when the experimenter wished to,

[14] Sewall, *op. cit.*, 118.

[15] W. Köhler, Akustische Untersuchungen III und IV, *Zsch. f. Psychol.*, 64, 1913, 95 f.
F. C. Bartlett and H. Mark, A note on local fatigue in the auditory system, *Brit. J. Psychol.*, 13, 1922, 215-218.
R. Bleyl, Die funktionelle Ermüdung des Gehörorganes, *Arch. f. Ohren-, Nasen-, u. Kehlkopfheilkunde*, 108, 1921, 192-197.

make a tone in it, he closed a double-pole switch, which (1) connected the oscillator circuit with the wires leading to the telephone, and (2) closed a circuit through a relay, the effect of which was to break the short-circuit of that telephone. Small electric buzzers of the type used for the practice of radio signals were adapted for use as relays and attached to the backs of the telephones. The headset with these relays was rather heavy, but a counter-weight passing over a pulley was arranged to relieve the *Os* of the burden on their heads.

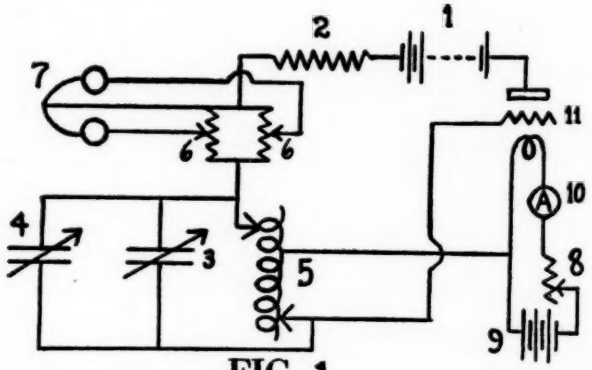

FIG. 1

FIG. 1. CIRCUIT DIAGRAM OF THE AUDIO-FREQUENCY OSCILLATOR. 1, 67 volt 'B' battery; 2, 5000 ohm resistance; 3, Dubilier step condenser, ten 0.001 mf. units; 4, General Radio Co. air condenser, 0.001 mf.; 5, tapped inductance, 3 lb. No. 30 D. C. C. wire; 6, 400 ohm potentiometer; 7, telephones; 8, 6 ohm filament rheostat; 9, 6 volt 'A' battery; 10, filament ammeter; 11, Western Electric Co. 216-A vacuum tube.

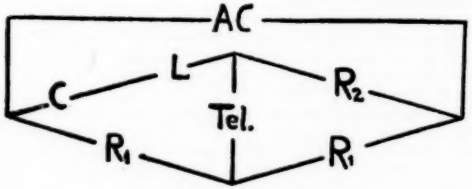

FIG. 2

FIG. 2. DIAGRAM OF AUDIO-FREQUENCY BRIDGE. The output of the oscillator is received at AC. C, capacity; L, inductance; R, resistance; Tel., telephone.

The oscillator was calibrated for frequency by means of an audio-frequency 'bridge' (Fig. 2) similar to the Wheatstone bridge for the measurement of resistance. This divided circuit, with constant capacity C and resistances R_1 and R_2 (the values of R_1 and R_2 being chosen to balance with the resistance component of the LC circuit), but with variable inductance L, received the output of the oscillator of unknown frequency at AC, and the inductance L was then varied until the tone heard in the telephone was of minimum loudness. With this value of L the apparent resistance to the alternating current of the branch of the circuit containing C and L balanced R_2. The value of L was now read off and the frequency of oscillation found from a curve plotted from the formula $f = \frac{1}{2\pi\sqrt{LC}}$.

Since the tones of this oscillator were not pure, the need was felt for confirmation of the results with pure tones. The Scientific Equipment Division of the Western Electric Company very kindly loaned one of their 2-A Audiometers (which produce pure tones by means of electric oscillations) for this purpose. This instrument furnishes 8 tones at intervals of an octave, beginning with 64 and ending with 8192 v. These tones are approximately as pure as tuning-fork tones of the same intensity.

A further advantage of this instrument is that the intensity of the tones is accurately calibrated in terms of 'sensation units', *i.e.* when the energy of the tone is increased by one sensation unit the initial energy is multiplied by the Fechnerian ratio.[16]

Method of Investigation

The fatigue was investigated by means of a comparison of the intensities of a tone of constant energy[17] and frequency when heard (1) by a normal ear, and (2) by the same ear after being subjected to prolonged stimulation. This comparison was made by a method which may be described as follows:

Let E_l and E_r represent the left ear and the right ear, respectively. Two tones of the same pitch, symbolized by T_l and T_r, were used as stimuli for E_l and E_r, respectively. The two tones were presented successively, T_l always to E_l and T_r always to E_r, each for about one second, for comparison as to intensity. The energy of the tones was so arranged by the experimenter that in a small percentage of the cases the intensity of T_r was judged greater than that of T_l. Not less than 50 of these preliminary judgments were taken. The instructions given *O*, who was seated where he could not see the adjustments of the apparatus, were: "Compare the intensity of the tone heard in the left ear with that of the tone heard in the right ear, and report whether the intensity of the tone last heard is greater than, equal to, or less than that of the tone first heard." Other pairs of tones of the same pitch but of rather pronounced differences in intensity were interspersed among the T_l-T_r pairs, in order to keep *O* alert, but only the judgments on the T_l-T_r pairs were recorded.

After these judgments had been taken with both ears in a normal condition, E_l was stimulated by the fatiguing tone F_l (often T_l and F_l

[16]A more detailed definition is furnished by the handbook accompanying the Audiometer as follows: "When the dial is rotated, thus changing the voltage at the receiver terminals, the number of sensation units corresponding to this change is equal to 20 times the common logarithm of the voltage ratio. At any given pitch, the pressure on the ear is proportional to the voltage. The pressure on the ear is therefore changed by a ratio of 10 to 1 when the dial is rotated through 20 sensation units. The energy absorbed by the ear is changed by a ratio of 10 to 1 by rotating the dial through 10 units. The smallest steps are 5 sensation units, corresponding to a pressure ratio of 1 to 1.77 or an energy ratio of 3.16." (The pressure of a tone is directly proportional to the square root of its energy.)

[17]Hereafter in this paper the word 'intensity' refers only to sensation and not to stimulus, for which 'energy' is employed.

were the same) for 1 min. At the end of the minute F_l was cut off, and the tones T_l-T_r were given successively for comparison immediately thereafter and also at intervals of 10 sec. during a time ranging from 30 to 100 sec. after the cessation of F_l. After every pair *O* reported his judgment on the intensities of T_l and T_r, and the judgment was recorded by the experimenter. After F_l had been given once for the minute-period and the subsequent series of intensity-judgments taken, the experimenter then had recorded one judgment taken immediately upon the cessation of F_l, one judgment taken 10 sec. afterwards, one taken 20 sec. afterwards, and so on. The fatiguing tone was now given again and another series of judgments at 10-sec. intervals taken, and this process was repeated 15 or 20 times in the course of the sitting.

Since E_r was not subjected to any prolonged stimulation, it may be considered, for the purpose of this experiment, as unfatigued. Hence the intensity of T_r in E_r was treated as a constant element in the experiment and served as a standard against which the intensities of T_l (1) when E_l was fatigued and (2) when E_l was unfatigued were measured. The intensity of T_r was the middle term which allowed these two intensities of T_l to be compared and a fatigue effect was thus shown, if present, by a decrease in the intensity of T_l after prolonged stimulation of E_l.

This method may seem rather indirect, in that the fatigue is measured by the *O*'s reports indicating a relative increase in the intensity in E_r, the unfatigued ear, rather than by reports showing a decrease in intensity in E_l. The merit of the former procedure lies in the probability that it is less open to the influence of suggestion: if *O* were asked to report on the intensity in E_l he might naturally expect it to be less after a minute's stimulation, and this expectation would be a complicating factor in his judgments; whereas, in order to gain an expectation of an increase in the intensity in E_r, *O* would have to think over the experimental situation, a less likely possibility.

Variations of this fundamental method are discussed in a subsequent section.

The data obtained by this procedure may be presented either in tabular or in graphic form. The results of one experiment, in which the left ear of *C* was fatigued for 1 min. by a tone of 512 v. at an intensity obtained by setting the potentiometer dial[18] at 85, are shown in the graph in Fig. 3. The ordinate represents the percentage of judgments in which the intensity in the right ear was judged greater. The abscissa is marked off in units of 10 sec. and represents the time after the cessation of F_l. What the graph is designed to show is the percentage of judgments of 'greater intensity on the right' as a function of the time elapsed since the cessation of F_l. The dotted horizontal line which is drawn through the 18% line is called the 'normal line' and represents the percentage of judgments of 'greater on the right' which occurred when, at the beginning of the experimental hour, the *O* compared T_l and T_r under normal conditions 100 times. Each point plotted on the curve is derived from 20 judgments after stimulation of E_l. This particular curve is one of the smoothest obtained in this investigation.

The data upon which this curve is based are given in tabular form under Fig. 3. The same method of tabulation is used in Tables I to IV. The italicized numbers under the column heading 'Energy' represent sensation unit values on the audiometer; plain numbers represent readings on the potentiometer dial of the oscillator.

When the effect of a loud tone was to be compared with that of a weak tone, the loud tone was first used to stimulate the ear for 1 min., and a series of judgments was taken immediately afterward. Then the weak tone was used as a fatiguing stimulus for 1 min. and a similar series of

[18]Large numbers on the potentiometer dial of the oscillator corresponded to loud tones, and conversely.

judgments taken. The two tones were thus used alternately until the end of the experiment. The purpose of this alternation was to insure that any changes occurring in either *O*'s condition or the apparatus during the experimental hour would be reflected in both fatigue curves. The same method

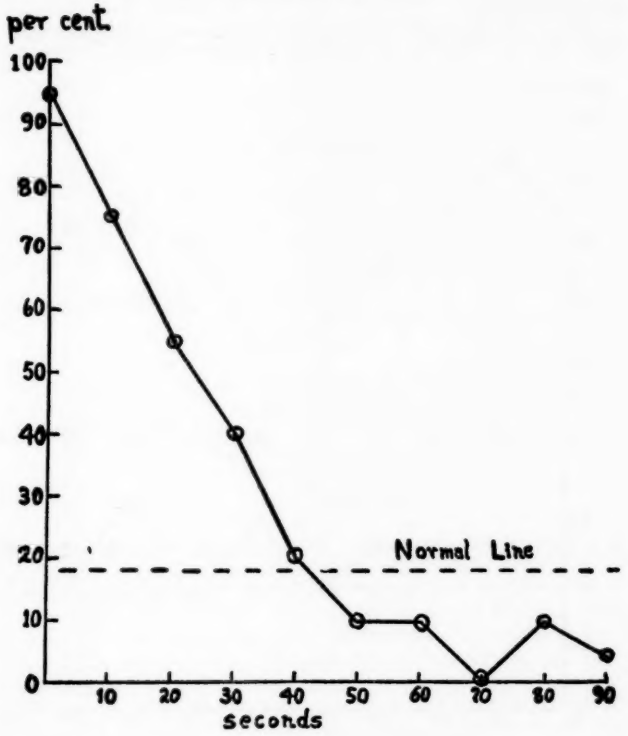

FIG. 3. GRAPH OF RECOVERY FROM FATIGUE, ILLUSTRATING THE RESULTS OF ONE EXPERIMENT. (See text.)

Tabulation:

		Fatiguing tone F						Percentage of judgments 'greater on unfatigued side' at										
O	Ear	pitch	energy	duration	T-T, pitch	Normal line at	No. judgments	0	10	20	30	40	50	60	70	80	90	sec.
C	L	512	85	1′	512	18	20	95	75	55	40	20	10	10	0	10	5	

of alternation was used when the effect of increased duration of the fatiguing tone was tested and also when the influence of different pitches was investigated.

Observers

Thirteen *Os* were employed, 4 at Harvard in 1923-1924 and 9 at Princeton in 1924-1925.

The curve of fatigue shown in Fig. 3 represents the typical form of the curve which was eventually given by all *Os* except one. Every one of

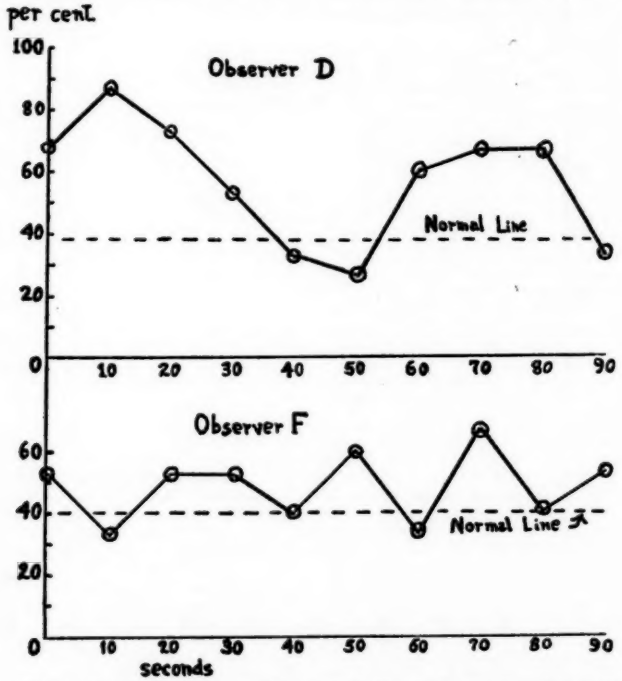

Fig. 4. Sample Curves of Observers *D* and *F*. Every point on the curves is determined by 15 judgments. *D* finally gave curves like the one in Fig. 3, while *F*'s curves continued to be as irregular as the one here shown.

the *Os* of the first group required considerable practice before his results on different days attained consistency among themselves and agreement with those of the other *Os*. The second group (at Princeton) did not require any practice; the curves given at the first hour they served were of the same kind as that in Fig. 3.

The *Os* of the first group were: *F*, *A*, graduate students, and *C* and *D* senior students. *F* and *C* were men; *A* and *D* were women. Every *O*

worked for about 15 weeks, coming once a week for an experimental period of two hours. *C* and *A* acted as *Os* for about 5 weeks before they began to give the usual form of curve. After they began to give this curve, they continued to do so the rest of the year. The results of the work with *F* and *D* were very irregular. Samples of curves given by them are shown in Fig. 4. Their results showed no consistency except that the curves were usually above the 'normal line.' These two *Os* were not discarded, however, and finally, during the last two experimental periods of the year, *D* gave four curves of the usual form. *F* never gave a curve of the usual form—his remained like those in Fig. 4.

The members of the second group of *Os* were: *G*, professor; *B*, instructor; *K*, *L*, *M*, graduate students; and *H*, *J*, *E*, *I*, senior students—all of the Department of Psychology. These *Os* were employed for varying lengths of time and at rather irregular intervals.

Why the difference as to the need for practice existed between the two groups has never been clear. The apparatus and method, except for the addition of the audiometer in the second year, were the same in both years. With respect to *D*, it seems that the failure to obtain agreement with the other *Os* for so long a time might be due to her lack of interest in the experimental work, which she may have regarded merely as means of obtaining college credit. This explanation does not hold for other members of that group of *Os*, and no adequate explanation (in terms of conditions lying outside the *Os* themselves) has been found.

Results

The results of the experiments are recorded in Tables I to IV, inclusive. A study of the Tables yields the following facts concerning the fatigue-effect:

(1) *The degree of fatigue.* The maximum amount or degree of fatigue was found by subtracting the percentage of 'greater' judgments in the T_l-T_r comparisons before fatigue was established (represented in the tabulated data as the 'normal line') from the greatest percentage of 'greater' judgments which occurs after the cessation of the fatiguing tone. This method can be applied only to those series where the maximum percentage after fatigue is less than 100; for it is obvious that, once a sound is judged greater in 100% of the cases, its intensity may then be increased indefinitely and the percentage of 'greater' judgments will not reflect this change in intensity.

The subtraction mentioned was made in 53 series of results in Tables I, II, and III where the fatigue was produced by stimulation for 1 min. with a fairly loud tone (about 100 on the oscillator dial and 75-105 sensation units on the audiometer). The arithmetic mean of the increase in 'greater' judgments on the unfatigued side was found to be 57%; the median was 60%, with a mean deviation of 16%; *i.e.* the decrease in intensity of a tone due to fatigue under these conditions is, on the average, somewhat more than would be necessary to constitute a difference limen under usual conditions.

(2) *The duration of fatigue.* The method employed in these experiments will furnish only an approximate result with respect

TABLE I. THE NON-SPECIFICITY OF FATIGUE

In this and following Tables, L and R, under the column headed 'Ear,' stand for 'left' and 'right;' in other places, G, E, and L stand for 'greater,' 'equal,' and 'less,' respectively. Pitch is given in d.v. Where the oscillator was used, the numbers given under the column headed 'Energy' refer to readings on the potentiometer dial, and such readings for T_l and T_r are omitted because of the oscillator's lack of energy-calibration. Where the audiometer was used, the numbers under the 'Energy' column refer to sensation-units and are italicized. For further explanation of the method of tabulation, see the text referring to Fig. 3 and the tabulation under Fig. 3.

			F			T_l-T_r		Normal Line at	No. Judgments	Percentage of judgments 'greater' on unfatigued side' at								
No.	O	Ear	Pitch	Energy	Duration	Pitch	Energy			0	5	10	20	30	40	50	60	sec
1	*A*	L	627	100	1′	512		20	15	100	—	67	20	0				
2	*B*	L	256	*85*	1′	2048	*70*	14	15	100	27	27	13	0	7			
3	*B*	R	256	*85*	1′	2048	*70*	8	15	60	53	47	20	7	7			
4	*B*	L	1024	*80*	1′	256	*70*	24	20	95	45	30	25	5				
5	*E*	L	512	100	1′	4500		20	15	73	67	47	53	27	13	13		
6	*E*	R	1024	*90*	1′	256	*55*	34	15	93	—	93	73	40	33	47		
7	*E*	L	512	100	1′	1330		34	16	94	69	81	69	69	31	13		
8	*E*	L	256	*85*	20″	2048	*60*	12	15	60	27	13	0	0				
9	*J*	L	512	100	1′	4500		10	15	93	87	73	53	33	40			
10	*J*	R	256	*85*	1′	1024	*70*	30	16	100	94	81	56	56	50			
11	*J*	L	1024	*95*	1′	2048	*80*	42	20	65	50	50	45	15				
12	*L*	R	4096	*80*	1′	256	*85*	38	15	87	93	73	47	40	40			
13	*M*	L	256	*65*	1′	1024	*70*	28	15	47	40	20	7	20	7			
14	*N*	L	512	100	1′	1330		22	15	100	100	100	87	67	60	40	27	
15	*B*	L	512	100	1′	512		12	15	80	—	73	53	20	33	13		
			550							27	—	40	7	20	13	13		
16	*B*	L	512	100	1′	512		18	15	40	—	53	27	20	7	20		
			550							80	—	40	27	13	7	7		
17	*B*	L	512	100	1′	512		18	15	93	—	80	73	33	40	20		
			550							87	—	60	47	33	20	33		
18	*B*	L	512	*85*	1′	512	*70*	16	15	80	53	27	13	27	13			
			1024							67	53	27	20	13	7			
19	*C*	R	512	100	1′	512		20	15	93	—	80	33	0	0			
			627							100	—	93	13	0	0			
20	*C*	R	512	100	1′	512		38	15	100	—	80	53	0	0			
			627							33	—	0	0	0	0			
21	*C*	L	512	100	1′	512		13	15	100	—	60	0	0	0			
			627							93	—	60	13	0	0			
22	*C*	L	512	100	1′	512		14	15	100	—	80	87	47	0			
			1330							93	—	53	20	0	0			
23	*K*	L	512	100	1′	512		0	15	93	87	40	27	27	7	13		
			550							73	87	87	40	27	27	27		
24	*K*	L	512	100	1′	512		0	15	60	—	47	53	40	20	0		
			550							100	—	60	20	20	7	7		
25	*K*	R	256	*85*	1′	2048	*70*	0	15	40	33	27	27	20	20			

to the duration of fatigue, since one point on a fatigue curve (or one value in the series tabulated) is determined by only 15 or 20 judgments, and these judgments were taken at intervals of 10 sec.

The duration was measured by taking the time required, after the cessation of the fatiguing stimulus, for the fatigue curve to reach the 'normal line,' *i.e.* the recovery-time. This measurement was made in the series in Tables I, II, and III where the fatigue was produced by stimulation for 1 min. with a tone of

an energy equal to 75-105 sensation units on the audiometer (or a tone of 512 v. at intensity 100 on the oscillator).

The time of recovery in 51 series of this sort ranged from 10 to 60 sec. The median time was 33 sec.; the mean was 30 sec., with a mean deviation of 10 sec.

TABLE II. THE EFFECT OF THE ENERGY OF THE FATIGUING STIMULUS
(See legend of Table I)

No.	O	Ear	F Pitch	F Energy	F Duration	Tl-Tr Pitch	Tl-Tr Energy	Normal line at	No judgments	Percentage of judgments 'greater on unfatigued side' at 0	5	10	20	30	40	50	60	70 sec
1	*B*	L	512	100	1′	512		16	15	73	73	67	60	33	27	33		
				20						60	60	40	40	40	47	40		
2	*B*	L	512	90	1′	512		24	15	47	—	53	47	7	7	13		
				30						27	—	20	0	13	20	13		
3	*B*	L	512	100	1′	512		20	15	93	67	47	40	27	20	7		
				20						27	40	7	7	27	13	7		
4	*B*	L	1024	*90*	30″	1024	*75*	10	20	80	30	25	10	0				
				70						65	25	0	0	5				
5	*B*	L	1024	*86*	30″	1024	*70*	4	20	75	30	20	15	0				
				70						75	50	15	10	20				
				60						35	10	5	5	0				
6	*C*	L	512	100	1′	512		35	15	100	—	93	100	67	47	7	0	0
				20						33	—	40	20	20	7	0	7	0
7	*C*	L	512	100	1′	512		20	15	100	—	100	93	87	60	27	13	7
				20						73	—	80	47	27	13	0	0	0
8	*C*	L	1330	100	1′	512		14	15	93	—	53	20	0	0			
				20						33	—	27	7	13	0			
9	*C*	L	512	100	1′	512		36	15	100	—	93	93	87	67	27	7	0
				20						93	—	80	60	27	13	7	7	0
10	*D*	L	512	100	1′	512		10	15	67	—	40	20	7	7	27	7	27
				20						20	—	13	13	13	20	13	27	20
11	*I*	L	1024	*90*	30″	1024	*70*	*	15	G 67	53	27	7	— no 'less' judgments —				
				70						G 73	40	27	0	— only 2 'lesses' —				
				60					20	E 60	60	55	50	— no 'greater' judgments —				
12	*I*	L	512	100	1′	512		18	15	100	93	93	60	33	33	40		
				5						53	47	33	27	33	27	27		
13	*K*	L	512	100	1′	512		20	15	80	60	60	7	0	0	0		
				20						20	20	7	7	7	0	7		
14	*K*	L	1024	*105*	1′	1024		0	20	95	25	5	0	0				
				75						70	30	0	0	0				
15	*M*	L	512	100	1′	512		58	15	93	60	40	27	27	27	33		
				10						40	20	20	27	20	20	27		

* 36% 'equal,' 64% 'less.' Tl—Tr given in order left-right.

In 9 series in these Tables the 'normal line' was not reached within the 40- or 50-sec. limit of the series, and they were not considered in the above calculation on account of their indeterminateness.

Significant individual differences in the duration of fatigue do not appear. Twelve series of *B*, 12 of *C*, and 6 of *K* gave mean durations of 28, 34, and 33 sec. respectively. The mean deviations were 7, 10.5, and 11 sec., respectively.

(3) *The non-specificity of fatigue.* The results exhibited in Table I show, without exception, that fatigue produced by a

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tone of a given frequency affects the intensity of all other tones used in the experiments. The tones used as fatiguing stimuli were 256, 512, 550, 627, 1024, 1330, and 4096 v. The comparison stimuli were 256, 512, 1024, 1330, 2048, and 4500 v. The series obtained when fatigue is produced by a tone of a pitch different from that of the comparison stimuli show, when plotted as curves, the same form as those curves obtained when fatiguing and comparison stimuli are of the same pitch.

Ten sets of results, Nos. 15 to 24, inclusive, embrace two lines of figures. These lines represent 2 series which were obtained in the same experimental hour for the purpose of comparing the degree of fatigue set up by a tone of 512 v. with that set up by some other tone. The comparison stimuli were in all cases 512 v. These data were all obtained by the method of alternation previously described.

In these 10 series, the effect of the other tone is approximately equal to that of 512 in 5 (17, 18, 19, 21, 23), less than that of 512 in 3 (15, 20, 22), and more than that of 512 in 2 (16, 24).

From these double sets of results and from the other series in Table I, the conclusion may be drawn that any of the tones used is as effective a cause of fatigue as any other one used in this investigation.

This finding does not cast any light upon a theory of hearing, since the seat of the fatigue is yet unknown.[19] The fact of non-specificity merely eliminates the statement, formerly accepted as a fact supporting the resonance theory, that the fatigue is confined to the pitch producing it.

(4) *The dependence of fatigue upon the energy of the stimulus.* In Table II 15 sets of results are given, wherein the effect of a loud sound is compared (in the same hour) with that of the same sound when diminished in energy.

Without exception these results show that less fatigue is produced by a weak tone than by a strong one, *i.e.* the fatigue is less and recovery from it is quicker.[20] A considerable difference in energy is required if a difference in fatigue-effect is to be shown, *e.g.* in Nos. 5 and 11 the tone 1024 v. produces the same result at 90 or 95 sensation units as it does at 70, but at 50 sensation units a markedly less effect is shown.

(5) *The dependence of fatigue upon the duration of the stimulus.* In Table III are gathered 18 sets of results in which the effect of increased duration of the fatiguing tone is tested. Here the results are not very clear cut. In 5 sets the effect of a 1-min. period is compared with that of a 2-min. period. In 4

[19] It is unlikely that the *tensor tympani* is concerned in the fatigue, since its reflex contraction is consensual. Cf. W. Köhler, *Zsch. f. Psychol.*, 54, 1909, 250.

[20] Nos. 1, 10, 11 and 12 form an exception to this last clause, since the values are very irregular at the ends of the series.

TABLE III. THE EFFECT OF THE DURATION OF THE FATIGUING STIMULUS

(See legend of Table I)

No.	O	Ear	F: Pitch	F: Energy	F: Duration	T_l-T_r: Pitch	T_l-T_r: Energy	Normal Line at	No. judgments	Percentage of judgments 'greater on unfatigued side' at 0	5	10	20	30	40	50	60	70	80	90	
	A	L	512	100	2′	512		17	12	100	—	100	83	58	50	33	25	25	17	17sec.	
					1′					100	—	75	58	8	8	17	0	0	0	8	
2	A	L	512	100	2′	512		12	12	100	—	100	75	58	50	50	25	17	17	17	
					1′					100	—	75	58	17	0	8	8	8	0	0	
3	C	L	512	90	2′	512		21	15	93	—	87	73	60	47	7	0	0			
					1′					73	—	67	27	7	0	0	0	0			
4	C	L	512	100	2′	512		12	15	93	—	67	20	0	0	7	0	0			
					1′					93	—	80	33	0	0	0	0	0			
5	K	L	512	100	2′	512		14	15	87	—	80	47	27	20	7					
					1′					60	—	47	33	27	33	40					
6	H	L	512	100	1′	512		7	15	87	67	47	13	7	0						
					30″					87	73	53	27	7	27						
7	J	R	256	85	1′	1024	70	30	16	100	93	81	56	56	50						
					30″					88	75	56	38	19	50						
8	L	L	512	100	1′	512		34	15	100	87	73	67	47	67	40					
					30″					100	87	80	73	80	67	73					
9	B	R	256	85	1′	2048	70	8	15	60	53	47	20	7	7						
					20″					60	67	73	7	13	7						
10	B	L	1024	80	1′	256	70	24	20	95	45	30	25	5							
					20″					85	50	35	15	20							
11	B	L	1024	70	1′	1024	70	0*	15	73	40	20	0	20							
					20″					87	80	20	7	0							
12	E	R	1024	90	1′	256	55	34	20	70	70	55	30	25	35						
					20″					50	55	40	25	25	20						
13	E	L	256	85	1′	2048	50	12	15	47	33	27	7	0							
					20″					60	27	13	0	0							
14	H	R	1024	85	1′	1024	75	0*	15	L 60	33	27	13	0							
					20″					L 33	7	7	0	0							
15	I	L	1024	100	1′	1024	70	0*	15	60	40	7	0	0							
					20″					27	13	7	0	0							
16	J	L	1024	95	1′	2048	80	42	20	65	50	50	45	15							
					20″					60	35	40	20	40							
17	K	L	2048	80	1′	2048	80	0*	20	90	35	5	0	0							
					20″					70	15	5	0	0							
18	L	R	2048	85	1′	256	70	88	15	G 13	20	67	93	93	87						
					20″					LG 60	27	0	0	0	0						
										LG 27	53	87	87	80	87						
										L 27	7	0	0	0	0						

* All equal.

* 12% less. No 'greaters' in the whole experiment. Comparison stimuli given in order left-right.

* 22% equal, 78% less. No 'lesses' given except in judgments before fatigue.

* All equal.

Comparison stimuli given in order left-right, right judged in 88% of cases greater. Right ear fatigued.

instances stimulation for 2-min. gives a greater effect than for 1 min., while in the remaining series the effect of the shorter period is very slightly greater.

Of the 10 sets of series comparing the effects of 1-min. and 20-sec. stimulation, the longer period gives a greater effect in only 6 sets, in one set (No. 10) the effect is practically the same in both, and in Nos. 11, 13, and 9 (a very irregular curve) the 20-sec. stimulation gives more fatigue.

The remaining 3 sets of data deal with periods of 1 min. and of 30 sec. In Nos. 6 and 8 the effect is greater for the shorter period of stimulation, but longer stimulation gives greater fatigue in No. 7.

To summarize: the longer period gives the greater fatigue in 11 cases, less fatigue in 6, and no significant difference in 1 case.

If, however, we consider the results obtained immediately after the cessation of the fatiguing tone (entered in the column headed '0 sec.'), we find the shorter period producing the greater fatigue in only Nos. 11 and 13. The value under '0 sec.' is undoubtedly the most reliable point on the curves, since the *O*s said that the judgments then were easiest.

From the facts given above, it may be concluded that when periods of stimulation shorter than 2 min. are used, a period of 1 min. usually produces less fatigue than one of 2 min., and a period of 20 or 30 sec. usually produces less than one of 1 min.

(6) *An apparent sensitization-effect.* In Table IV are given 5 series from 3 *O*s which seem to indicate that occasionally a very weak tone, when used as a stimulus for 1 min., has an effect opposite to fatigue, *i.e.* its effect is to make subsequent tones of greater rather than of less intensity.

No. 1 in the Table shows in the first line the usual fatigue produced by a loud tone. The only judgments made in this part of the experiment were 'greater' and 'equal.' The next three lines show the judgments after the left ear had been stimulated with the same tone at a much lower level of energy. The result of the weak stimulation was to eliminate the 'greater' judgments entirely. The *O* (Professor Langfeld), well trained in such observation, reported 'less' and 'equal' only—never 'greater.' In the first members of the series, the 'less' judgments predominated, but they gradually diminished as time passed and gave way to increasing numbers of 'equals.'

No. 2 is of the same type as No. 1, but not so clear cut.

Nos. 3 and 4 are cases where the only effect of the weak tone was to depress the percentage of 'greater' judgments below the 'normal line' instead of raising it above in accordance with the usual effect. In No. 5, with the same *O*, 'less' judgments occurred for the first time in the experimental hour after the ear had been subjected to weak stimulation.

The writer is restrained, however, from concluding that the effect of weak stimulation is to sensitize the auditory mechanism, because his efforts to produce the sensitization-effect subsequent to its first appearance have in practically all cases failed. He has tried many times to produce the effect, but the result has been slight fatigue; the appearance of the sensitiza-

TABLE IV. AN APPARENT SENSITIZATION-EFFECT

(See legend of Table I)

No.	O	Ear	F: Pitch	F: Energy	F: Duration	Tl-Tr Pitch	Normal Line at	No. judgments		Percentage of judgments at 0	5	10	20	30	40	50	60	70 sec.	
1	G	L	512	100	1′	512	20	15	G	100	73	73	47	7	0	0			No 'lesses.'
				20					G	0	0	0	0	0	0	0			
									L	93	80	73	13	20	0	7			
									E	7	20	27	87	80	100	93			
2	L	L	512	100	1′	512	42	15	G	93	60	47	53	60	47	53			
									L	0	7	20	7	27	20	20			
									E	7	33	33	40	13	33	27			
				20					G	13	13	20	27	53	73	47			
									L	60	33	20	20	0	13	7			
									E	27	54	60	53	47	14	47			
3	A	L	512	100	1′	512	60	15	G	100	—	93	67	13	7	7	0	0	No 'lesses.'
				20					G	7	—	0	7	0	0	0	0	0	
4	A	L	512	100	1′	512	26	15	G	100	—	100	93	73	33	27	13	7	—do.—
				20					G	0	—	0	0	7	0	7	0	7	
5	A	L	512	100	1′	512	16	13	G	61	—	13	0	0	0	0	0	0	No 'lesses.'
				50					L	62	—	46	31	15	31	46	46	46	No 'greaters.'

tion-effect has been on rare and unexpected occasions, when he was not trying to produce it. It is this feature of the effect which prevents him from drawing a conclusion, and not any suspicion of the actual data, which were obtained throughout by the method of alternation and with as much care as any of the other data in this paper.

Variations of the Fundamental Method

One of the objects of the investigation was to vary the conditions of the experiment in such a way as to insure, so far as possible, that the fatigue-phenomenon was of physiological, and not of psychological, nature.[21] As an example of a phenomenon of psychological origin, we may consider one of the facts reported by Sewall: if, after giving a fatiguing tone on the left side, he now threw in the tone on the right (without cutting off the left tone), the tone on the right seemed much more intense. He attributed this effect to an attentional factor; the tone on the right began as something new and changing, attracting attention to itself, and was therefore judged to be of greater intensity. The 'psychological' nature of the effect was shown by the fact that if he interrupted the fatiguing tone for one or two seconds before presenting the two tones simultaneously, the 'fatigue' effect vanished completely.

It might be objected that similar factors were at work in this experiment; after the fatiguing tone had been heard for a minute and was then cut off, the tone on the opposite side beginning immediately thereafter may, by virtue of its novelty, possess more clearness than the fatiguing tone. The relative durations of the tones may play a part also in conditioning judgments of intensity; the longer tone, being monotonous, may lose clearness. Of two tones differing only in clearness, the clearer might be judged the more intense, since (1) clearness and intensity are, to a certain extent, covariants and (2) the *O*s employed may not have been sufficiently trained to abstract from clearness in making their judgments.

The usual method of investigation was, therefore, varied in order to eliminate as far as possible any psychological influences which might be giving a false appearance of fatigue and in order to see if the fatigue-effect would disappear in other experimental settings.

The methods used were four:

(1) When F_l was the same tone as T_l the method was: E_l was stimulated with F_l. Simultaneously with the cutting off of F_l, without any appreciable interval, T_r was given in E_r. The *O* was instructed to compare the intensity of that part of F_l heard after the experimenter's ready-signal (given about 2 sec. before the end of F_l) with the intensity of T_r.

[21] Cf. footnote 3.

This procedure was used with *A*, *C*, *D*, and *F* whenever the fatiguing tone was the same as the comparison tone on the same side.

(2) This method was the same as (1) except that between F_l ($=T_l$) and T_r an interval of about 1 sec. intervened. This plan was followed in order to eliminate the shock, so to speak, of the very quick change from F_l in one ear to T_r in the other, a change which might be accompanied by an increase in clearness of the experience as the second tone comes suddenly into consciousness.

This method was used with *B*, *E*, *G*, *H*, *I*, *J*, *K*, *L*, *M* whenever F_l and T_l were the same.

(3) This method was employed when F_l and T_l were different in intensity or pitch or both. After F_l had been heard in E_l during the fatiguing period, it was cut off, and after 1 sec. T_l was given in E_l; then, after another pause of 1 sec., T_r was given in E_r.

This procedure was followed with all *O*s whenever the tones F_l and T_l were not the same.

In (2) and (3) both of the short tones T_l and T_r appeared as new stimuli with respect to the long fatiguing stimulus and were separated from each other and from the fatiguing stimulus by short intervals. It seems that, if clearness accrues to short sensations because of their novelty, then both T_l and T_r should receive equal increments. If there should be an unequal distribution of clearness due to the temporal order of the short tones, then the order should be changed to eliminate this factor. This change was made in Method 4.

(4) In this method, the usual order of presentation of T_l and T_r was reversed, and the stimuli were given in the order T_r-T_l. This method was used in the following series: Table I, Nos. 6 and 25; Table II, No. 13; Table III, Nos. 6 and 8; Table IV, No. 1.

It is apparent from an inspection of the series obtained by means of these different methods that the fatigue-effect appears in all of them and that no significant differences are shown by the results.

Since the first work with most of the *O*s consisted in obtaining the fatigue-effect when the fatiguing stimulus was of the same pitch as the comparison tones T_l and T_r, it might be objected that the *O*s acquired a 'set' or bias from this earlier work which led them to give the same results when the fatiguing tone and the comparison tones were of different pitches. This objection is met by Nos. 5 and 9 of Table I (transfer of fatigue produced by 512 v. to the intensity of tones of 4500 v.), which were obtained on the first occasion that each of these *O*s served.

The persistence of the fatigue-effect through all these changes of conditions affords ground for considering it to be of physiological not psychological nature. The facts, too, that there are no significant individual differences among the *O*s and that the results of any one *O* show consistency among themselves, afford further ground for believing that the phenomenon is conditioned by physiological changes caused by prolonged stimulation and is not due to a distortion of judgment produced by attentional factors, psychological contrast, or the like; for in the latter case a greater variability and instability of the fatigue-effect would not unreasonably be expected. Of course, no absolute proof of

the physiological nature of the effect can be given from the facts here set forth; the effect can only be shown to occur persistently in a variety of experimental settings.

Summary

(1) The result of prolonged stimulation of the auditory mechanism is a diminution of the intensity of the stimulating tone and of tones subsequently heard. This effect is called fatigue in this paper.

(2) This decrease in intensity amounts to somewhat more than one difference limen, when the stimulating tone is 1 min. in duration and of the energy corresponding to 75-100 sensation units of the Western Electric audiometer.

(3) The duration of this fatigue-effect is about 30 sec. under the conditions of paragraph (2) above.

(4) The fatigue is non-specific, *i.e.* the intensity of tones of pitch different from the stimulating tone is decreased by the latter.

(5) The degree of fatigue produced by a weak tone is less than that produced by a strong tone.

(6) The amount of fatigue produced by stimulation for 2 min. is usually more than that produced by stimulation for 1 min. Stimulation for 1 min. usually produces a greater amount of fatigue than stimulation for 20 or 30 sec.

(7) The fatigue-effect persists when the experimental setting is varied in such a way as to eliminate, so far as is possible, attentional or other 'psychological' factors.

AN EXPERIMENTAL STUDY OF THE DIFFERENTIATION OF TEMPERAMENTS ON A BASIS OF RATE AND STRENGTH[1]

By MILDRED FRANCES BAXTER

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I. INTRODUCTION

As Allport has said in regard to the classification of temperaments, "the four-humor doctrine has haunted the house of psychology for twenty-five centuries."[2] During recent years a new version of this four-fold classification has been put forth. Bahnsen,[3] Wundt,[4] Ribéry,[5] and others have suggested that the four temperaments are a quick-strong, a quick-weak, a slow-strong and a slow-weak type. This classification has repeatedly found a place in text-books as a possible basis for the grouping of individuals with respect to temperament. We have in this research endeavored to determine whether or not this system of differentiation is justified experimentally.

First we shall try to make clear what is meant by our terms quickness or rate and strength. Writers have not agreed on their significance. Bahnsen[6] refers to quick or slow receptivity and to strong or weak spontaneity, Wundt[7] to the quickness and strength of the emotions, and Ribéry[8] to strong and quick motor

[1] From the Psychological Laboratory of the University of Michigan.
[2] G. W. Allport, Personality and character, *Psychol. Bull.*, 18, 1921, 445.
[3] W. Stern, *Die differentielle Psychologie in ihren methodischen Grundlagen*, 1911, 482.
[4] W. Wundt, *Grundzüge der physiologischen Psychologie*, III, 1903, 638.
[5] W. Stern, *op. cit.*, 484.
[6] *Loc. cit.* [7] *Loc. cit.* [8] *Loc. cit.*

and sensory reaction. The terms quickness and rate, as we have used them, require little explanation. The time required for the performance of a task has been taken as the speed index. The situation is very much more difficult when we attempt to determine what has been meant by the term strength and how it shall be defined for the present purpose. There seems to have been no uniformity, and it has been applied to various traits. We learn from Stern[9] that Dorsch speaks of strong or weak bodies and minds, Herbart of strong or weak excitability and manifestation of the emotions; that Hellwig uses these terms in connection with reception and reaction, Kreibig with respect to the will, Klages with regard to motive power and resistance, and Ach with reference to the determining tendency and sensory-motor excitability. According to the interpretation that we have put on the term, strength is that element in efficiency which remains when the speed or rate factor in a performance is disregarded; *i.e.* quality or excellence.

Our aim, then, has been to ascertain whether there appear to be four types of individuals based on combinations of the rate and strength factors, and, in case they do seem to exist, whether they correspond to the types of temperament indicated by descriptions secured through questionaries.

II. Experimental Procedure

A. General

In attacking the problem we first secured 3 sets of scores or measures for both rate and strength. We then endeavored to determine whether any interrelation exists. The first set of scores was obtained from the results of a series of experiments which were selected with a view to including as great a variety of performances as possible. The second set of measures were physiological in character, and included pulse rate as a speed score, systolic blood pressure as a strength score, and temperature. The third set of scores was secured through a group of questionaries. These, in addition to judgments of the rate and efficiency of an *S* in performance, also yielded scores for the speed with which emotions were aroused and for their intensity, of the tendency to be cheerful or to be depressed, for the degree of activity in which *S* usually indulged, and for the tendency to be steady or to fluctuate.

The 138 young women who served as *Ss* in this research were university students who were at the time enrolled in the elementary course in psychology. No one of them was acquainted with the purpose of the investigation. Two groups are considered. Group I, composed of 90 *Ss*, was used for trying out methods and standardizing materials and procedure, preliminary to the selection of experiments to be used in gathering the final

[9] *Op. cit.*, 481 ff.

data from Group II. The coefficients of reliability, except for the tapping and carbon-copy experiments, and the physiological measures were computed from data obtained from this group. Group II comprised 48 *Ss*, from whom 44 complete sets of data were obtained. A set was considered complete when scores were secured for every experiment, where no more than one of the six questionaries had to be omitted (there were two cases of one omission), and where the physiological measures were secured for at least one morning and one afternoon period. The age range for the members of Group II, with the exception of one *S* aged about 40, was 16 to 25 yrs.; the mode was 19.

The work with Group I extended over a period of 2 years, though that with any one individual was usually completed within 2 weeks. Group II worked for six one-hour periods between March 25 and June 2, 1923. The first and third periods were morning hours, the second and fourth afternoon hours; the last two came in the evening. The first two periods were arranged so that an interval of 28 hr. intervened. It was found to be impossible to carry out this plan quite as rigidly for the third and fourth hours. In 9 of the 44 cases from 2-5 days elapsed between these periods, and in one case 9 days. The evening hours came exactly one week apart.

With Group II the experiments were conducted individually, except for part C of the carbon-copy experiment and for the filling out of questionaries C and D, where the work was done in groups of from nine to fifteen. The procedure was identical for all *Ss*, the order for the first four periods being: (1) experiments in a fixed sequence, (2) physiological measurements, (3) questionary B. Four of the experiments were divided, for the purpose of obtaining sets of scores from which coefficients of reliability might be computed, into two parts which were given on different days. The parts of the carbon-copy, clock problem, and maze experiments were given during the first two periods, those of the tapping experiment during the second and third hours. Where an experiment yields more than one score we shall describe the apparatus or material and general procedure when the experiment is first referred to; the method of obtaining other scores will be treated later.

B. Experiments

(1) *For Rate Measures.* These experiments vary in the degree to which speed was required, ranging from tapping and the learning of nonsense syllables in as few trials as possible, where speed was the only element of interest, through experiments such as the cancellation of words, where the effort was to be divided between speed and accuracy, to those where the rate was purely voluntary. Another aspect in which they are different is the amount of motor activity required in their performance and the degree of mental organization involved. We have attempted to consider these factors in listing and grouping the experiments, placing the emphasis on the first in cases where the two conflicted.

(*R-1*) *Tapping Experiment. Apparatus.* We used a tapping board in the form of an oval brass plate mounted on a $6 \times 4 \times \frac{1}{8}$ in. block, and a tapping stylus with flexible connecting wire attached. For recording the number of taps an Ewald's chronoscope[10] was placed in the circuit. Before beginning experiments in March we tested the chronoscope by comparing the number of taps recorded by it with the number simultaneously recorded on a kymograph. Light, heavy jarring, and glancing strokes were all tried. The chronoscope was adjusted until the two records, for all but glancing strokes, agreed.

Procedure. Position and method were demonstrated. *S* sat with right arm (except in the case of one who was left-handed) resting comfortably on

[10]Ewald's chronoscope was used without tuning fork as an electric counter.

the table, elbow touching but wrist free. We were careful to see that the stylus was held erect to avoid glancing strokes and was firmly held so that adjustment would not be necessary during the performance. The instructions were: "Tap as rapidly as possible from the signal 'now' to the signal 'stop' which will be given about one half minute later. Pay attention only to your tapping." The switch was opened and stop-watch started as soon as *S* began to tap, and closed at the end of 30 sec. with the giving of the 'stop' signal. Two trials with a one-minute rest interval were given at the beginning of the second and third hours. The average of the first two was correlated with the average of the second two for an index of reliability, and the average of all four trials used for the score. The coefficient of reliability computed from results from 44 members of Group II was found to be .878 ± .0198.[11]

(*R-2*) *Simple Reaction-Time to Sound. Apparatus.* The Hipp chronoscope, a relay, sound hammer, and telegraph key, were set up according to the Dunlap break-to-break system. The initial adjustment of the chronoscope for absolute time and constancy was made with a control pendulum. While the experiment was in progress checks for accuracy were made, at intervals during each day, with a gravity control apparatus which was always in the circuit.

The average m.v. for the test series of 10 times previous to the beginning of an experiment was never more than .6σ and usually much lower, even as low as .0σ. This m.v. was for a time interval of about .483σ and included any error introduced by the control apparatus.

Procedure. We explained the purpose of this experiment to *S* and made her familiar with the stimulus and mode of reacting before the experiment was begun. The interval between the 'ready' signal and stimulus was 2 sec. Thirty reactions were taken. This number was thought to be sufficient for this and for the association reaction-time experiment, on a basis of preliminary work done with Group I. Eighteen series of 50 reaction-times were secured from this group. When the m.v. was calculated for each of these series and for the first half of each series the coefficient of correlation between the m.v.'s for the long and short series was found to be .941 ± .019. Thirty reactions were taken instead of 25, since 30 was the number used by Woodrow[12] in his experiment for the measurement of attention, which we have included in our series of experiments for securing strength scores. Occasionally there were reactions unquestionably premature, and long ones due to lapses of attention, which it was thought should not be included in the average time. The method of eliminating these errors was to omit arbitrarily all reactions below 100 σ and the three longest in every series. The coefficient of reliability, computed from pairs of scores obtained one week apart from 31 *Ss* in Group I, is .589 ± .079.

(*R-3*) *Association Reaction Time Experiment. Apparatus.* An Ach card-exposer apparatus was connected in place of the sound hammer used in the simple reaction-time experiment, and a lip key[13] in place of the telegraph key. A series of 30 familiar words was constructed, including 18 nouns, 6 adjectives, 3 words which might be understood as adjectives or nouns, and 3 which might be nouns or verbs. Only one had as many as 6 letters, and 4 as many as 3 syllables. The words were typed in small letters in the center of white cards $3\frac{1}{8} \times 1\frac{5}{8}$ in. Five blank cards were

[11]The formulae used for computing all coefficients and their probable errors are, unless otherwise stated, the familiar product moments formulae.

[12]H. Woodrow, The measurement of attention, *Psychol. Rev. Mon.*, 17, 1914, No. 5.

[13]This key was found to be less satisfactory than was anticipated for it was extremely difficult to get all *Ss* to react in the same manner. Though the instructions were to release the key as the word was spoken, some had a tendency to get the lips away from the key before speaking the word and so were in danger of letting go even before an associate had occurred to them, while others tended to retain their hold on the key while they spoke the word.

introduced at irregular intervals during the series, as a means of detecting any tendency to release the key at the sight of the stimulus word instead of waiting for the appropriate response.

Procedure. *S* was instructed to hold the key closed between her lips until the word exposed had suggested another word but to speak this word as soon as it came to mind. The procedure was demonstrated: 2 practice trials in responding with the first associate were given without the apparatus; then a series of 3 trials using 2 cards with words and a blank one between, was given with the apparatus. Whenever a word in the regular series was reacted to prematurely or while the key was kept closed, it was repeated after several others had intervened. The average of the 30 satisfactory reactions was used as the score. To determine the coefficient of reliability, a second series of words agreeing exactly with the first in regard to the number of nouns, etc., and number of letters and syllables, and similar with respect to meaning, was constructed and given to 31 members of Group I one week after they had had the first list. The coefficient was found to be .698 ± .062.

(*R-4*) *Learning of Nonsense Syllables. Apparatus.* A modification of the Wirth exposure apparatus, arranged by Professor Shepard, was used. The syllables of the series, fastened to a revolving drum, were exposed in succession in an opening 1⅛ × 1⅝ in. A series of 8 nonsense syllables was used;[14] two series were constructed.[15]

Procedure. Series I was used with Group II. The syllables were exposed at the rate of one every 2 sec. *S* was required to spell the syllable aloud during the exposure, as Luh suggested, in order to prevent the forming of meaningful associations. Immediately after every exposure of the series *S* wrote all the syllables that she could recall. The exposures were repeated until the series was written correctly twice with no exposure between these recalls. Frequently one recall would be perfect, but errors introduced into the second reproduction of the list would indicate that the learning was not complete. The rate score was the number of exposures required. Series I and II were learned a week apart by 37 members of Group I. The scores obtained gave a coefficient of reliability for the rate of learning of .663 ± .062.

(*R-5*) *Carbon Copy or 'Accuracy of Movement' Experiment. Part B—Speeded Time Material.* A complete description of the material will be given here, though the chief purpose for which it was arranged was to get a strength score which will be considered later. The time scores, *R-5*, *R-13*, *R-14*, were secured incidentally and served the purpose of hiding from *S* the real purpose of the experiment. Ten sheets of unglazed onion-skin paper, 8⅛ × 5⅛ in., and 10 sheets of light weight black carbon paper were alternately arranged, the top carbon covered by a sheet of graph paper, and the whole set slipped into a white cardboard frame. The

[14] Originally the lists consisted of 12 syllables, but the time required to learn this number was prohibitive.

[15] The series were constructed according to the following plan:
(1) Every syllable was composed of an initial consonant, a vowel, and a final consonant.
(2) No consonant was used twice as the initial or final letter in the same series.
(3) The initial or final consonants of consecutive syllables were not adjacent letters in the alphabet.
(4) No vowel was used more than twice in the same series.
(5) No vowel was used more than once with the same consonant in any of the 16 syllables.
(6) No given vowel followed another given vowel in successive syllables more than once.
(7) No syllable was used that was found to have meaning according to English, Latin, French, or German dictionaries.
(8) Consecutive syllables did not form polysyllabic words.
Rules 1, 2, 6, and 8 are Müller and Schumann's as quoted by E. Meumann (*Psychology of Learning*, trans. by J. W. Baird, 1913, 365 f.); 7 was suggested by C. W. Luh (The conditions of retention, *Psychol. Rev., Mon.*, 31, 1922, No. 142, 3); the others are believed to be new.

right-hand side of the frame was left open when working with individuals, where *E* could prevent handling by *S*; for group work the pads were sealed along that edge also. The graph paper was ruled in light blue, 6 squares to the inch. A black ink margin was drawn just outside the overlapping edge of the frame, and the area within it was divided into sections, one each for *S*'s name, the date and title of the experiment, and three parts, A, B, and C, of the experiment. In each of the last 3 sections three sets of squares, 25 in a row and 2 in height, with a row between sets, were outlined in red. Everything possible was done to keep the apparent but false purpose of the experiment, *i.e.* the measurement of accuracy of movement, before *S*. Uniform No. 3 pencils, kept strictly for this experiment and sharpened after each using, were used by all *S*s in every part of the experiment. In no case so far as is known was the real purpose suspected.

Procedure. The instructions for Part A, *i.e.* for the voluntary time experiments, are given here, since those for part B depend on them. They were: "The purpose of this experiment is to determine how exactly you can control your hand movements. In Part A you are to make three lines of small and large loops, a loop between every pair of blue lines. The small loop is to be one space high, the large loop two spaces high. The line connecting the loops should just touch the lower red line. Be sure to touch the boundaries but do not go beyond them. Begin the first line with a small loop; it will end with a small one, so begin the second line with a large one, and then the third with a small loop. Take your own time; speed does not count." A pattern set of loops was shown to avoid any error. The instructions for Part B, which was given as soon as *S*'s hand was rested, were: "In Part B do exactly the same thing, except that this time do it just as quickly as you can. This time both accuracy and speed count equally. Ready—go." The stop-watch was started with the 'go' signal and stopped as *S* completed the final loop. This time is the score used for speed experiment *R-5*. Group II went through this experiment twice with a 28-hour interval between performances; *i.e.* it was given them during the first and second of their periods. The results for both performances were summed for the scores in all parts of the carbon copy experiment. The coefficient of reliability, computed from the two sets of scores for part B, was .897 ± .0199.

(*R-6*) *Maze Experiments.* It was thought that the kind of performance required in this experiment might bring out differences among the *S*s in their tendency to plunge ahead, sacrificing all else for speed, or to exercise caution.

Material: Two rather simple forms of maze, quite similar to those used with a very different procedure in learning experiments conducted in this laboratory, were employed. Form I was given during the first period and Form II on the second day. The times for the two were averaged for the final score.

Procedure: The instructions were: "I shall give you a maze similar to this one. [A third form was used for demonstration.] When I say 'go, start with your pencil at A (at the point of the arrow) and find your way through; you will come out at B. Keep your pencil on the paper from start to finish. Avoid such blind alleys as this. If you start to go into one I shall say 'no;' when I say 'no,' come out immediately and choose another path. Find your way through as *quickly* and *carefully* as you can. The time you take and the number of wrong turnings count equally." The maze was placed and kept squarely in front of *S*. The stop-watch was started with the 'go' signal and stopped when *S*'s pencil reached B. *S* was required to go through the maze until two successive perfect tracings were made. The time was recorded for all trials, though the first two on each day were the only ones included in the final score, which was an average of these 4 times. The practice-effect, where many trials were necessary, cut

the time to such an extent that the average time in those cases was not comparable with the average time of an *S* whose first two trials were without error. The time-scores for one performance with each of the two forms (one performance = 2 trials) by 80 *S*s from Groups I and II gave a coefficient of reliability of .629 ± .046; the coefficient of reliability, obtained from 31 sets of scores from Group I, where the scores correlated were the total time for the first performance of Forms I and II and the total time of a second performance, a week later, of both forms, was .808 ± .042.

(*R-7*) *Münsterberg Experiment.* This experiment is a repetition, as nearly as could be planned from the descriptions available,[16] of the one which Münsterberg employed in testing the efficiency of electric railway motormen.

Apparatus. A frame held a pack of 12 cards which represented streets. Each of these had lines through the center for car tracks and was scattered with black and red digits which stood for moving objects in the road. A belt, having an opening representing a motorman's window, revolved about the frame parallel to the tracks.

Procedure. *S* was informed regarding the assumed movements of the digits on the cards and was told to see how rapidly she could pass the belt over the card and still detect, through the exposure window, all sections of the track endangered by the moving objects. The space between the tracks was divided into 26 sections, each of which was lettered. In case a moving object and the car reached any section simultaneously its letter was to be called. *S* was warned that accuracy and speed counted equally and was given practice in securing these. *E* slipped each card from under the belt as soon as it had been exposed. The time was taken from the exposure of row A on the first card to the covering of row Z on the last card. This time-score for 30 of Group I for two trials a week apart gave a reliability coefficient of .882 ± .027.

(*R-8*) *Cancellation Experiment. Material.* The passages used for this and other experiments to be mentioned were adapted from a small volume by Dunbar.[17] All three selections as arranged contained 19 lines and 212 words. The *Antwerp*[18] and *Holy Week*[19] passages, which were used for the cancellation, have each 95 words to be cancelled. Those entitled *Caterina* and *Antwerp*, and used for the recall of ideas in the test for logical memory, were each divided arbitrarily into 100 'ideas.' To determine the comparative difficulty, for cancelling, of the first pair of selections, they were given on the same day to 27 members of Group I and again a week later to 23 of these. The resulting data indicated that the two selections were of approximately equal difficulty with respect to the time required and the number of errors made. The *Caterina* and *Antwerp* passages were tested in much the same way and accepted as about equal for reading time and difficulty in the recall of ideas.

Procedure. *S* was required to read the selection aloud, word by word, not by phrases, and cancel every word as she came to it, that had an 'a' or an 'e' or both those letters in it providing there was no 'i' or 'o' in the word. The sense of the passage was to be disregarded. Speed and accuracy were emphasized equally. The purpose of the oral reading was to rule out as far as possible the tendency to reread any parts of the selections which were to be read later for recall. The plan was to use the cancellation performance with the *Holy Week* passage as a control for the cancellation with *Antwerp* where the added task of attending to the sense for the purpose of recall was to be the only changed factor in the experiment. The

[16] H. Münsterberg, *Psychology and Industrial Efficiency*, 1913, 68 ff.; Marx Lobsien, Prüfung der Aufmerksamkeit an Kindern mit der Münsterbergschen Schlittenmethode, *Zsch. f. angew. Psychol.*, 13, 1918, 395 ff.

[17] M. L. Dunbar, *Felicilla's Easter*, 1903, 7 f. [18] *Ibid.*, 13 f. [19] *Ibid.*, 1 f.

time required to complete the task was the score used. The results obtained by giving the *Holy Week* selection twice, a week apart, to 33 members of Group I give a coefficient of reliability of .883 ± .026 for this time. When the time-scores of the first of the two above sets were correlated with the time-scores secured on the same day with the *Antwerp* passage the coefficient is equally high, .884 ± .025, although in the second performance there was introduced a factor not included in the first.

(*R-9*) *Clock Problem Experiment. Apparatus.* Two series, each of 6 clock problems such as Terman [20] uses in his intelligence scale, were used with the exposure apparatus by which the nonsense syllables were shown.

Procedure. The instructions were arranged to suggest the use of visual imagery rather than the seeking for and application of some mechanical means of arriving at the answer. As *S* spoke the answer to the problem exposed the next one was moved into place. The time was taken for the entire series. As previously stated one series was given in each of the first two periods. The rate score is the total of the times required for both series. Two more series were constructed and given a week after the first to 31 members of Group I. The rate scores for the first two and last two series were correlated and gave a coefficient of reliability of .794 ± .045.

(*R-10*) *Mystery Problem Experiment. Material.* The plan of this experiment is similar to a single part of the Burt reasoning test.[21] The mystery story, for which possible solutions have to be selected, is an adaptation from a Scandinavian story by Ingeman.[22] It comprised a single type written sheet. In the final form 14 solutions are suggested on the answer sheet, of which 6 are possible and 8 impossible according to the evidence presented. The first step in standardizing was to seek an arrangement of the material and a procedure that would prove satisfactory from the points of view of scoring and time required. The second step was to compare the results obtained by this procedure with those secured in 3 other tests requiring the thinking through of a situation or problem. At first *S* was required to suggest all possible solutions that occurred to her and signify the one regarded as most probable. This method gave results extremely difficult to score and required too long a time. Next 25 solutions were listed from which *S* selected those she considered possible. In this form the experiment was given twice, a week apart, to 21 members of Group I. We tried various methods of scoring and finally settled temporarily on the following. Only 14 of the 25 solutions were scored. These included the 6 possible ones, 4 which involved one of the 2 impossible conditions, *i.e.* either wounds on the body or the presence of a second person, and 4 which necessitated both of these. Points were given as follows: +1 for every possible solution checked, −1 for every impossible one. The one selected as most probable was given an additional score of from +1 to +6, according to the degree of probability, if a possible solution, and −1 if it involved one of the impossible conditions or −2 if both were required. When the data for the coefficients of reliability for the rate and strength scores had been gathered it was found advisable to make a slight change in the scoring. We gave a score of +4 if no solution involving either of the impossible conditions were checked, −2 if any solution checked involved one of these impossible circumstances, and −4 if both were required by anyone of the solutions checked. To this was added the number of points received for the doubly checked solution. This latter method was used in treating the results on which the coefficients of reliability are based and with the final data; the former method in all other calculations.

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[20] L. M. Terman, *The Measurement of Intelligence*, 1916, 321.

[21] C. Burt, The development of reasoning in school children, *J. Exper. Ped.*, 5, 1919, 73 ff.

[22] B. S. Ingeman, *The Sealed Room*, 1, 1909, 271 ff.

The tests used for comparison were the Burt Reasoning Test,[23] Syllogism Test D,[24] and the arithmetic problems in Test 5 of the Terman Group Test of Mental Ability, Form B.[25] Two sets of 15 problems from the Burt scale were arranged by the writer and Miss Kurtti to be given by the group method. For the most part only the more difficult problems were used. These were selected to include the widest variety of situations possible, and were divided into 2 balanced sets.

The four tests to be considered were given to 22 members of Group I; the first as an individual test, the others by the group method. Only 10 members of this group could be secured for a second performance of the tests a week later. The coefficients of reliability for the scores were:

Mystery Problem	.835 ± .063
The Burt Reasoning Test	.685 ± .115 (Forms I, II)
Syllogism Test D.	.850 ± .058
Arithmetic Problems	.798 ± .075

To investigate the possibility of a factor common to all 4 tests and to determine to what extent this factor entered into the solving of the mystery problem, every score in the 4 sets was reduced to terms of the standard deviation for the set of which it was a part. This score was expressed as a percentage of the deviation and marked (+) if above the average score and (−) if below it. The 4 reduced scores thus obtained for *S* were averaged for a composite score. The 4 sets of reduced scores were then correlated, using the method described by Woodworth,[26] with this set of composite scores and yielded the following results:

Mystery Problem and Composite Score	= .712 ± .067
Burt Reasoning Test (Form I) and Composite Score	= .781 ± .057
Syllogism Test D and Composite Score	= .630 ± .087
Arithmetic Test and Composite Score	= .685 ± .077

We believe these results indicate the existence of a common factor and justify the use of the Mystery Problem experiment for investigating it.

Procedure. We placed the selection to be read and the answer sheet face down on the table before *S*, the story at her right, then read the following instructions.

"I want to see how well you can think through this problem. It is a mystery to solve. You are to determine how Lieut. Ribot met his death. First read the passage straight through aloud. As soon as you have finished reading it turn this other paper over. There you will find suggested a number of solutions to the mystery. There is one *correct* solution; the others may be possible or impossible. You are to put a check before *every possible* cause of Lieut. Ribot's death and a second check before *the one* which you judge to be the *correct* solution. Before you check the solutions weigh carefully the evidence presented throughout the passage. Part of the evidence points to the one correct solution, part to other possible solutions, and part is misleading, suggesting solutions which *the facts* given show to be impossible. Accept all *statements of fact* as true. You may refer to the passage while checking. Work quickly and carefully. Tell me just as soon as you have finished. Are there any questions? Now begin: read the passage aloud."

We required oral reading to prevent the immediate rereading and studying of sections by some *S*s, and thus insured a more uniform method of attack. The stop-watch was started when *S* began to read the passage and

[23] Burt, *op. cit.*,

[24] *Syllogism Test D*, Dept. Educ. and Psychol., Carnegie Institute of Technology, 1921.

[25] L. M. Terman, *Terman Group Test of Mental Ability*, 1920.

[26] R. S. Woodworth, Combining the results of several tests: A study in statistical method, *Psychol. Rev.*, 19, 1912, 97 ff.

stopped when she reported that she had finished, or, since the instruction to report was frequently forgotten, when she started to hand the paper over or looked up indicating that she was satisfied with the placing of her second check. The coefficient or reliability for the rate score based on 32 cases is .628 ± .072.

(*R-11*) *Voluntary Reading Time.* This rate score was secured as incidental to the strength score *S-9*, and was the rate which *S* voluntarily selected as her usual rate for oral reading.

Material. The *Caterina* selection mentioned under *R-8* was the passage read.

Procedure. For the instructions, see experiment *S-9*. The coefficient of reliability, based on 58 cases, is .852 ± .024.

(*R-12*) *Judgment of Weights, Voluntary Time.* We followed rather closely Whipple's[27] description of Binet's test for depersonalized suggestion by progressive weights.

Materials. A set of 15 weights, of identical size and appearance, and numbered conspicuously from 1 to 15 were used. The first four weighed 42, 63, 84 and 105 grm. respectively; the remaining 11 each weighed 126 grm. These were arranged as numbered in a row, about 2 cm. apart, on a thick black cloth, No. 1, the lightest, at the left, and the eleven heaviest, at the right.

Procedure. The instructions were: "Here is a series of weights, 15 of them. You are to lift them, one after the other, like this, with thumb and fore-finger of the right hand to the height of the weights (10 cm.). As you lift each weight you are to say whether it is heavier, lighter, or the same as the one just before it. Compare each weight as *you lift it.* Here is the first weight, No. 1 at the left of the row." *S* did not know that a record of her rate was being taken; time was not mentioned unless *S* waited for a signal to start; in that case *E* told her to start when ready, that time did not matter. The stop-watch was started when *S* lifted the second weight and stopped when the judgment on No. 15 was spoken. The time was measured from the second weight because some *S*s paused to ask a question while holding the first one. The coefficient of reliability for the rate of judgment, based on 62 pairs of scores, is .711 ± .043.

(*R-13*) *Carbon Copy Experiment. Part A — Voluntary Time.* The material and procedure have been treated in detail under *R-5*. The rate score is the time required for the task. Before this experiment was undertaken at all we endeavored to determine how constant *S*'s 'own time' or 'usual rate' as selected by her would be, and what effect would be exerted on such a rate by one or more speeded experiments preceding it. Twenty-seven members of Group I served as *S*s, though complete records were not obtained from all. Task 1 was to write the numbers 21 through 35, *i.e.* 30 digits; task 2 to write the letters of the alphabet *a* through *z* and again through *d*, *i.e.* 30 letters. Coefficients of reliability were obtained as follows:

(1) Where the 2 performances of task 1, at the voluntary rate, were separated by a short interval, but one in which the same task had been done as rapidly as possible and task 2 both at a voluntary and speeded rate, r = .937 ± .025.

(2) Where the second performance came at the end of the hour, so that a number of tasks, usually at a speeded rate, had been done in the meantime, r = .896 ± .032.

(3) Where the second performance followed the first after an interval of one week, both trials coming at the beginning of the hour, r = .845 ± .05.

(4) Where the first performance came at the beginning of the first hour and the second at the end of the second hour, a week later, r = .796 ± .06.

[27] G. M. Whipple, *Manual of Mental and Physical Tests*, II, 1915, 232.

We thought it safe to conclude that an *S*'s choice of her 'own time' or 'usual rate' was sufficiently constant to be relied upon. The coefficient of reliability for the voluntary rate score, based on the 44 cases in Group II, justified this decision; it was + .813 ± .034.

(*R-14*) *Carbon Copy Rate Ratio.* This ratio, obtained by dividing the speeded time score by the voluntary time score, is used as an index of the degree to which *S* fails to work at the speed of which she is capable, or of the degree to which she sacrifices speed to accuracy. The ratio scores from Group II give a coefficient of reliability of .628 ± .062.

(*2*) *For Strength Measures.* (*S-1*) *Strength-Weight Ratio,* (*st/wt*). The muscle strength test used to obtain the strength score for this ratio was worked out and partially standardized by Lovett, Martin, and Rich.[28] It measures with a dynamometer the maximum resistance to a steady pull which is sufficient to overcome the contraction of the muscle group under consideration. That this is not a measure of the total potential strength of the muscle tissue, and not, taken alone, an index of physical condition, is evident. Besides the factors of the amount and of the quality of muscle tissue, as Martin and Rich[29] point out, there enters in the factor of the degree to which *S* is able and, it may be added, willing, to innervate the muscles. Rather than the utmost positive effort, the value obtained for each muscle is the 'breaking strength,' the tension shown on the scale at the instant the resistance of the contracted muscle is overcome by a pull in the opposite direction, exerted through the balance. In the complete scale 22 pairs of muscles were measured.[30] Martin and Rich[31] found, however, that a score could be computed for this entire group, from measures secured by testing 4 of the 22 pairs of muscles, which gave a coefficient of correlation of .94 ± .01 with the score gained by actually testing all 44 muscles. The procedure used in this experiment was the abbreviated one by which the strength of the right and left pectoral, forearm flexor, thigh adductor, and thigh abductor muscles was measured.

Apparatus. A light weight flat-face spring balance with a scale capacity of 200 lbs. by 2 lbs. intervals was equipped with a self-registering device. A wooden handle was attached by a swivel to the upper end of the scale and a woven strap attached in a loop about 30 in. in circumference by another swivel to the lower end of the balance. A stout, rather narrow, table with a cleat firmly fastened across one end and a cushion for *S*'s head were necessary for the last 3 pairs of the measurements. A door frame was used for *S* to lean against in the first part, and to aid *S* in steadying herself a rope with a wooden handle was attached to the wall about 10 ft. to the side of *S*.

Procedure. *E* was assisted by an adjustor whose duties were: (1) to place the loop in the assigned position about the arm or leg; (2) to support the loop with one hand and if necessary the arm or leg with the other hand; and (3) to give the commands 'hold back' to mark the beginning of the pull and 'stop' to mark the end. *E* held the handle of the balance in her right hand and the body of the balance in her left. At the command, 'hold back,' *S* contracted the muscle group being tested, and simultaneously *E* pulled the spring-balance, developing tension as rapidly as possible without jerking, and increasing it until the resistance of *S* was overcome. At the 'stop' signal the pull was discontinued, the scale read and recorded. Two trials were given with each group of muscles and the higher of the two measures used. Detailed directions for testing the 4 pairs of muscles

[28] R. W. Lovett and E. G. Martin, Certain aspects of infantile paralysis; with a description of a method of muscle testing, *J. Amer. Med. Ass.*, 66, 1916, 729 ff. Cf. also E. G. Martin, Muscular strength and muscular symmetry in human beings: I. In children, *Amer. J. Physiol.*, 46, 1918, 67 ff.; and E. G. Martin and W. H. Rich, Muscular strength and muscular symmetry in human beings: II. In adult males, *ibid.*, 47, 1918, 29 ff.

[29] *Ibid.*, 39 f. [30] Lovett and Martin, *op. cit.*, 730. [31] *Op. cit.*, 34 ff.

are given by Martin and Rich.[32] The strength of these 8 muscles is equal to 15% of the entire strength by the complete test; therefore to calculate the total strength score, the sum of the measures for these 8 muscles was multiplied by the reciprocal of 15 or 6.67.[33] The ratio of the estimated total strength score to *S*'s weight measure taken at the beginning of this experiment was used as the final score. Martin and Rich[34] considered this a more reliable index, since a coefficient of correlation of .58 ± .04 was found between the strength score and weight. The coefficient of reliability of the ratio, based on 30 cases from Group I, is .749 ± .054.

(*S-2*) *Carbon Copy Score.* The purpose of this experiment is to measure the amount of physical force exerted as pressure during the performance of set tasks. The material and the procedure for part A and B have been taken up in detail under *R-5*. The procedure for part C and the method of scoring will be considered here. Part C was given to the subjects in small groups. The aim here was to set a task difficult to start with and increasing in difficulty to the point where there was little chance of its being done perfectly, so that the *S*s would be obliged to put forth every effort possible. For the first row of this part the instructions were: "This is another test for accuracy. Do it as in Part C. I shall dictate, rapidly, a series of digits which you are to write down as I give them, one digit between every pair of vertical blue lines. The height of the digits does not matter. Leave no spaces; there are 25 spaces and I shall dictate 25 digits. If you miss a digit, guess at it, for I shall not repeat any." *E* illustrated the rate, which was 2 per sec., then read the following series: 3754946281573681492385697. In the instructions for the next section, the *S*s were told, "This time I shall dictate letters rapidly. You are *not* to write the letter I say but the one that *follows* it in the alphabet. As before, write one between every pair of vertical blue lines. The height of the letters does not matter. Leave no spaces; if you are not sure of a letter guess at it." Here the rate was 1 letter per sec.; it was illustrated before the following series was read: aplgwcxnitfgyhobsjmudkvwe. The instructions for the last part were: "This time you are to write numbers. I shall give a number of two digits and you are to write the number which you get by subtracting one from the first digit and adding one to the second digit. Put one digit in each space, *i.e.* use 2 spaces for each number. Leave no spaces." To make sure the directions were understood the method was demonstrated on the blackboard with the number 58. This series was read at the rate of one every 2 sec. The numbers used were 72, 46, 60, 38, 81, 47, 28, 32, 97, 53, 25, 64.

The method of scoring was to count, for every part, including the heading where *S* wrote her name, the date, and Accuracy of Movement Test, the number of sheets of onion skin paper on which any of the carbon could be detected. Care was taken to do this in a bright but not glaring light. At times it was necessary to examine the carbon to differentiate between markings in the paper and carbon on its surface. If there was carbon on the second sheet under the heading, on the third under A, on the fourth under B and on the sixth under C, the score would be 15. As has been said, this experiment was given twice to Group II. The sum of both scores was used for the final strength score. The coefficient of reliability computed from these two sets, is .833 ± .030.

(*S-3*) *Woodrow's Measurement of Attention Experiment.*[35] This experiment may measure attention as Woodrow claims, or it may get at a more highly organized mental process, that of holding things (signal, stimulus,

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[32] *Op. cit.*, 35 f.

[33] L. M. Moore and J. L. Barker, Monthly variations in muscular efficiency of women, *Amer. J. Physiol.*, 64, 1923, 3, 405. In their work, these investigators multiplied the sum of the 8 measures by 7.04, since this sum has been found to be 14.2% of the total strength in the case of women where it is 15% for men.

[34] *Op. cit.*, 38 ff. [35] *Op. cit.*

reaction, etc.) in order, and measure *S*'s ability to carry out a type of plan, as Professor Shepard suggests. Which supposition is nearer the truth does not matter here, since we were satisfied that the experiment did give a measure of something not touched by the other experiments.

Apparatus and Procedure. This was just as in the simple reaction-time experiment which preceded it and furnished one of the 2 scores used in computing the final score. The instructions were: "In the next part of the experiment the interval between the 'ready' signal and the stimulus will vary in length. Otherwise it is identical with the first part. React *as quickly as you can*, just as soon as you hear the stimulus. Do your very best *every reaction*." The set of irregular intervals used was the one given by Woodrow.[36] Practice was given on the last 3 of the set before the times were recorded. The score is the absolute difference between the average reaction time for all the 2 sec. intervals and the average for all the irregular intervals. A low score is taken as an indication of strength. The coefficient of reliability for this score, based on 2 sets of 31 scores secured a week apart, was .425 ± .099.

(*S-4*) *The Maze Experiment.* The material and procedure have been given in detail under *R-6*. The strength score used was the total number of errors made in all trials with both forms. A coefficient of reliability of .402 ± .05 was obtained when 2 sets of 80 scores each were correlated. The scores in one set were the total number of errors made in all trials with Form I on the first day that it was given; those in the other set the total number made with Form II on the first day that it was given. Thirty-one *S*s worked with both forms of the maze again a week after their first performance with them. The coefficient found between the total number of errors made with Form I and II the first week, and the total number made the second week was .429 ± .099.

(*S-5*) *Münsterberg Strength Score.* Here again it is uncertain just what process or processes were being tested. It is probable that breadth of vision, perception, and attention—either its breadth or its flexibility—played a large part in the performance, which required the noting of the color, form, and locality of all the digits. The ability to construct speedily a systematic procedure quite probably increased efficiency to a great extent. The apparatus and procedure have been given in detail under *R-7*. The score used here was the total number of danger points that were omitted; though recorded, no attention was paid to the letters wrongly called. The coefficient of reliability, computed from 2 sets of 30 scores obtained a week apart, is .869 ± .031.

(*S-6*) *Cancellation Experiment.* An account of the material and procedure for this experiment has been given under *R-8*. The score used here was the total number of errors, both words wrongly cancelled and those omitted. The coefficients of reliability for this score, when computed for 2 trials a week apart, both using the *Holy Week* selection, with 33 *S*s, is .800 ± .042. When based on the first trial by 35 *S*s with *Holy Week* and the first trial with *Antwerp*, given on the same day, the coefficient of reliability is .875 ± .027, though a new factor, that of attending to the sense of the passage for the purpose of recall, entered into the latter task.

(*S-7*) *Clock Problem Experiment.* The material and procedure have been given under *R-9*. The score was the total number of problems in both series correctly answered. An answer was considered correct if it fell within the five minute interval; for instance, should the answer be 3:23, any answer between 3:20 and 3:25 inclusive would be accepted as correct. To determine the coefficient of reliability a third and a fourth set of 6 problems each were given, a week after the first sets, to 31 *S*s; it was found to be .638 ± .072.

[36] *Ibid.*, 127.

(*S-8*) *Mystery Problem Experiment.* The material, procedure and method of obtaining the strength score have been described under *R-10*. The experiment was performed a second time, a week later, by 32 *Ss*. They were instructed to go at the problem as carefully as though it were for the first time; they might arrive at the same conclusions or different ones. They seemed to carry out the directions quite faithfully, and many changes occurred in the second series of checkings, though the same types of solutions were considered possible. These data gave a coefficient of reliability of .502 ± .089.

(*S-9*) *Logical Memory Experiment. The Recall of Ideas. Material.* The *Caterina* selection mentioned under *R-8* was used.

Procedure. The instructions were, "This experiment is one to see how much of a selection you can recall after reading it through once. When I say 'go' turn the paper over and read the selection straight through aloud. Read it at your usual rate for oral reading. Put your attention on the sense rather than on your manner of reading. As soon as you have finished reading turn the paper over, and write as much of the passage as you can recall. Use the words of the passage as far as you can remember the exact wording; when you do not recall it, use your own words. Read it straight through once. Go." After *S* finished writing she was further directed to put down any other separate words or phrases recalled out of context.[37] An idea recorded was considered satisfactory if it approximated the original to a rather close degree. The experiment by Henderson[38] served as a guide in this respect. The score was the total number of ideas recalled. When the *Caterina* passage was given twice, a week apart, to 33 *Ss*, the scores gave a coefficient of reliability of .764 ± .049; when the scores for *Caterina* and for *Antwerp* were correlated, though the latter included a factor not in the former, *i.e.* cancelling, as with the *Holy Week* selection, while it was being read for recall, the coefficient of reliability for recall for 35 cases was .782 ± .044.

(*S-10*) *Suggestibility Experiment.* The material has been given under *R-12*. The score was the number of weights that were incorrectly judged as heavier. Here as in *S-3* a low score is considered indicative of strength. The coefficient of reliability for scores secured a week apart from 62 *Ss* is .567 ± .058.

We had planned for two other strength scores, but gave them up because of their apparent unreliability. The score for the written recall of the nonsense syllables after a lapse of 28 hours gave a coefficient of reliability of .105 ± .12. We had hoped to secure an index of the breadth or the flexibility of attention by having *S* cancel words in the *Antwerp* selection, as she had done with the *Holy Week* passage, *while* she read it straight through for recall. The ratios of the cancellation time, cancellation errors and recall scores in the single tasks, to the same scores in the double task were to have been combined for this final score. Although the lowest coefficients of reliability for any one of the 6 individual scores was .659 ± .066, the coefficients for the ratios were disappointing. The time ratio gave a coefficient of reliability of .595 ± .076, the error ratio a coefficient of −.018 ± .114, and the recall ratio one of .247 ± .110. It was evident that a score composed of ratios apparently so unreliable would itself have slight if any reliability.

(*3*) *Summary of Results.* We have expressed the individual scores in terms of rank, 1 signifying the highest place for rate

[37] She was also requested to underline every word that she was certain had occurred in the passage, but this score was not used, since we thought a low score might in some cases indicate weakness, as shown in lack of self-confidence, and in other instances strength, as evidenced by caution.

[38] E. N. Henderson, A study of memory for connected trains of thought, *Psychol. Rev. Mon.*, 5, 1903, 34.

and strength, 44 the lowest place. See Table I and II for sample cases.[39] Composite scores for every *S* for rate and strength were computed in two ways. One composite score was obtained by averaging the ranks secured in all tests of the rate series and

TABLE I
Rankings in Rate Experiments

S	R-1 Tap.	R-2 S.R.T.	Av. R. 1 & 2	R-3 A.R.T	R-4 N.Syl.	R-5 CC.S	R-6 Maze	R-7 Mun.	R-8 Can.	Av. 3, 4, 5, 6, 7, & 8
A	12.0	22.5	17.5	9.0	3.0	9.0	35.5	29.0	13.0	21.6
B	40.0	25.5	32.8	38.0	8.0	42.0	43.0	27.5	25.0	34.4
F	42.0	27.0	34.5	43.0	32.0	32.0	33.0	12.0	42.0	29.8
M	41.0	20.5	30.8	12.0	14.0	19.0	41.0	11.0	4.5	18.9

S	Av. R 7 & 8	R-9 Cl. P.	R-10 Myst.	R-11 R.T.	R-12 J.Wt.	R-13 CC.V.	Av. 11 12 & 13	R-14 CC/SV	Com. Score % Meth	Com. Score Av.R.
A	21.0	33.0	38.0	16.0	1.0	10.0	9.0	15.5	68	17.6
B	26.3	3.0	33.0	39.5	43.0	29.0	37.2	7.5	21	28.9
F	27.0	42.0	22.0	41.0	17.0	40.0	32.6	35.0	21	32.8
M	7.8	13.0	17.0	19.0	11.0	14.0	14.7	13.0	86	17.9

TABLE II
Rankings in Strength Experiments

S	S-1 S/W.	S-2 CC.	S-3 W's A.	S-4 Maze	S-5 Mun.	S-6 Can.	Av.R. 4 % 5
A	10.0	18.0	36.0	16.5	22.0	6.0	14.8
B	1.0	15.5	42.0	23.0	14.0	35.0	24.0
F	10.0	24.0	16.0	3.5	38.0	35.0	25.5
M	3.0	11.0	43.0	16.5	38.0	38.0	30.8

S	S-7 Cl P.	S-8 Myst.	S-9 R.Id.	Av. R 7, 8, 9	S-10 J. W.	C. Sc. %	C. Sc. Av.R.
A	39.0	16.5	7.0	20.8	14.5	80	18.6
B	15.5	16.5	1.0	11.0	6.5	75	17.0
F	31.5	25.0	33.0	29.9	27.5	35	24.4
M	39.0	33.5	21.5	31.3	41.0	35	28.5

again all in the strength series. For the other score the number of times that *S* gained a place in the quicker half of the group was expressed as a percentage of the number of possibilities of being in the group considered, *i. e.* as a percentage of the number of experiments in the series. The two forms of composite rate scores gave a coefficient of correlation of .894 ±.024, and those for the strength scores one of .859 ±.029. Because of the close

[39]Copies of Tables I, II, IV, and VII in their complete form are on file in the University of Michigan Library and may be borrowed under the usual courtesies of library exchange.

correlation only one, the percentage score, has been considered to any extent. Further presentation of the results and their discussion will be considered later.

C. Physiological Measures

Temperature, pulse rate, and blood pressure were taken at the end of the period with *S* seated. It was seldom necessary for *S* even to leave her chair more than once during the hour, so that the measures were taken after a comparatively long period entirely free from any muscular activity which would be likely to influence them. All *S*s had spent the time in performing the same mental tasks. They were comfortably seated beside the table on which they had been working and were instructed to relax, to rest, and "think about nothing in particular." The temperature was allowed to register in the mouth while *E* counted the pulse rate; blood pressure was taken last.

(*1*) *Pulse Rate.* *E* counted the pulse at the left wrist for two or three 30-sec. intervals to make sure that it was constant, and that the first rate had not been influenced by the newness of the situation. The rate for 30 sec. was doubled for the measure. The final rate used was the average of the 4 obtained during that number of periods. For the coefficient of reliability see Table III, and for samples of the measures, Table IV.

Table III
Coefficients of Reliability for Physiological Measures

Measured during	Pulse Rate	Blood Pressure	Temperature
A.M.	.691±.056	.679±.058	.212±.101
P.M.	.734±.049	.638±.063	.523±.076
A.M. + P.M.	.792±.041	.716±.054	.492±.084

Table IV
Physiological Measures

	Pulse Rate					Blood Pressure					Temperature				
S	AM_1	PM_1	AM_2	PM_2	Av.	AM_1	PM_1	AM_2	PM_2	Av.	AM_1	PM_1	AM_2	PM_2	Av.
A	84	84	74	74	79	104	96	96	92	97	97.2	98.6	98.2	98.4	98.3
B	80	76	76	60	73	92	90	96	96	94	98.2	99.0	98.4	98.6	98.6
F	96	96	92	86	91	100	88	80	84	88	99.0	99.0	97.6	98.0	98.4
M	74	76	76	68	74	110	102	104	106	106	96.6	97.4	97.6	97.4	97.3

(*2*) *Systolic Blood Pressure.* The Tycos sphygmomanometer was used with the auscultatory method. The compression sleeve was placed about the bare left arm just above the elbow, and the stethoscope applied over the radial artery at or above the elbow. Except in one or two instances where *S* was restless because of discomfort or haste, 2 or 3 readings were taken at each period, with rest intervals of at least a minute, for verification and to eliminate the possibility of the first rate's being high because of *S*'s unfamiliarity with the procedure. The measure used was the average of the 4 measures secured on 4 different days. For the coefficient of reliability see Table III, and for samples of the measures, Table IV.

(*3*) *Temperature.* A Tycos fever thermometer, requiring one minute for registration, was used. More than the required time was allowed and the reading taken at least twice to make sure that the temperature was not still rising. See Table III for the coefficients of reliability and Table IV for a few of the readings.

(*4*) *Summary of Results.* It will be noted (see Table IV) that the average temperature and blood pressure were low. Burlage[40] in a study of 1700 cases of public school and University girls found the average pulse rate for girls of 18 years to be 79.78 and to fluctuate little up to 26 years of age. Our average agrees closely with this. Schneider and Truesdell[41] found the average blood pressure of the group of normal women whom they were studying to fluctuate from 95 to 107 mm., with a midpoint of 101, for recumbency; and from 104 to 113.7 with a midpoint of 108.7, for standing position. Alvarez,[42] studying pressures obtained from 5807 women between the ages of 16 and 40 by palpation with *S* in standing position,[43] found the average for the entire age range to be 115 mm. This however, is higher than would be expected for our group, since he also found that, instead of the average pressure rising steadily with age as it had been supposed to do, it dropped between the ages of 17 and 25 years, which are the age limits that include all but 2 of our cases. The range for women proved to be practically 85 to 155; there were 4 cases, ages 16 to 21, with pressures between 80 and 84. We believe that our measures may be slightly low because taken after an hour of comparatively slight body activity but rather strenuous mental exertion. O'Hare,[44] in a study of normal and vascular hypertension cases, found that physical rest caused a marked fall in blood pressure, the range being from 0 to 46 mm. with an average of 21 mm. Pillsbury and Griffitts,[45] found that pressure also fell during a period devoted to the performance of mental tasks. It is possible also that the circumstance that the measures were all taken in the spring, during the last two months of the school year, was a contributing factor.

Since authorities do not seem to agree[46] on the presence or absence of a diurnal rhythm in blood pressure and pulse rate,

[40]S. R. Burlage, The blood pressures and heart rate, in girls, during adolescence: A statistical study of 1700 cases, *Amer. J. Physiol.*, 64, 1923, 259.

[41]E. C. Schneider and D. Truesdell, Daily variations in cardiovascular conditions and a physical efficiency rating, *Amer. J. Physiol.*, 67, 1923, 199.

[42]W. C. Alvarez, Blood pressure in university freshmen and office patients, *Arch. Int. Med.*, 26, 1920, 381 ff.

[43]The measures in this study would tend to be higher than ours because of the difference in *S*'s position, and on the other hand would tend to be lower because of the difference in the method used. These tendencies would probably just about compensate, making the data comparable with ours.

[44]J. P. O'Hare, Vascular reactions in vascular hypertension, *Amer. J. Med. Sci.*, 159, 1920, 370.

[45]C. H. Griffitts and W. B. Pillsbury, An experiment on indirect measures of fatigue, *Proc. VII. Inter. Cong. Psychol.*, 1924, 212 ff.

[46]Schneider and Truesdell, *op. cit.*, 194 ff. These investigators state that early students of pulse rate frequency came to the conclusion that it "is greater in the morning than in the evening," and that the heart was more excitable in the morning; also that Weysse and Lutz "concluded that the systolic pressure rises an average of 8 mm. immediately after the ingestion of food, then gradually falls until the beginning of the next meal, and that there is a general rise during the day, whereas their observations lend support to the more general increase in rate throughout the day and fail to indicate a decided diurnal change in the excitability of the heart," and show no general day-time rise in the level of arterial pressures for the groups that ate the usual meals, though such a gradual rise was found in a group that went without meals.

we averaged all measures taken at each hour from 9 A. M. to 5 P. M. The results are stated in Table V. The differences between the averages for measures taken at different hours were less than the variations found in many instances in measures taken for the same *S* at the same hour on two days. We concluded that it would therefore not be safe to associate unexpected findings in any case with the hour factor.

TABLE V
Distribution of Physical Measures by Hours

Hr.	Pulse Rate		Blood Press.		Temperature	
9	80 (*23*)		100 (*23*)		97.8 (*23*)	
	=79		=100		=98.0	
10	78 (*16*)		100 (*16*)		98.2 (*18*)	
		=79		=99		=98.2
11	80 (*25*)		98 (*26*)		98.1 (*26*)	
	=78		= 98		=98.3	
12	75 (*21*)		98 (*20*)		98.5 (21)	
2	83 (*24*)		100 (*24*)		98.1 (*22*)	
	=80		= 98		=98.2	
3	77 (*19*)		95 (*19*)		98.3 (*19*)	
		=79		=98		=98.3
4	81 (*21*)		95 (*21*)		98.4 (*21*)	
	=78		=97		=98.4	
5	74 (*20*)		98 (*20*)		98.4 (*20*)	

The figures in *italics* give the number of measures.

Another point on which there is disagreement[47] is that of the presence, (and nature, if present), or the absence of a monthly rhythm in cardio-vascular activities in women, and the presence or absence of a periodic mental and motor inefficiency. To determine whether or not this factor should be considered in our study, the average of all measures for every one of the 3 physiological processes was found for the menstrual period, for the 5 days previous to the onset, for the 5 days following the close, and for the inter-menstrual period. Our results, stated in Table VI, show slight differences for the 4 periods and are in

[47] L. M. Moore and C. R. Cooper, Monthly variations in cardio-vascular activities and in respiratory rate in women, *Amer. J. Physiol.*, 64, 1923, 416 ff. According to these writers the theory of presence of a rhythm was put forward by Jacobi in 1876 and held sway until investigations during the last decade in general produced evidence in apparent refutation. Their own study (*ibid.*, 420), however, and that by King (J. L. King, Concerning the periodic cardio-vascular and temperature variations in women, *Amer. J. Physiol.*, 34, 1914, 203 ff.) both support the rhythm theory, though they do not seem to agree concerning the order of the phases. King, as did Jacobi and others, found that there was "rhythmical movement in the life-processes," the highest point of which was reached from 2 to 4 days before the menses, the lowest point about 3 days after their cessation; whereas Moore and Cooper (*op. cit.*, 423) found that the typical variations are a low menstrual and a high inter-menstrual period. An investigation by Moore and Barker (L. M. Moore and J. L. Barker, Monthly variations in muscular efficiency in women, *Amer. J. Physiol.*, 64, 1923, 414) suggests a "menstrual decrease in strength." Another investigation, one by Hollingworth (L. S. Hollingworth, *Functional Periodicity*, 1914, 94) gives evidence in favor of there being no "periodic mental and motor inefficiency in normal women."

general agreement with the conclusion reached by King,[48] and by Jacobi and other early workers mentioned by King, rather than with the findings of Moore and Cooper.[49] However, when

TABLE VI

Distribution of Psychological Measures (averages) with Respect to Menstruation

	Pulse Rate	Blood Press.	Temp.
Intermenstrual period	76 (*77*)	97 (*77*)	98.0 (*78*)
Within 5 days of onset	82 (*33*)	100 (*33*)	98.5 (*33*)
Menstrual period	82 (*32*)	101 (*32*)	98.3 (*32*)
Within 5 days after close	77 (*20*)	97 (*20*)	98.0 (*20*)

Figures in *italics* gives the number of measures.

the records of our individual *S*s were studied, the directions of the variations concomitant with differences in physical condition were not consistent. Also similar differences were found between measures for the same *S*, all taken during the same type of period; and, on the other hand, identical measures were obtained for an individual at different phases of the cycle. We therefore concluded, as in the case of the distribution of measures by hours, that it would not be safe to associate unexpected findings with this periodic factor.

The record of physical examinations and medical attention received while attending the university was investigated for all *S*s, with special note of any reference to thyroid abnormality. Such record as there was did not indicate, however, the nature of the disturbance, so that it was impossible to tell whether one should expect pathological cardiovascular conditions, and in which direction any change which might be present had occurred.

D. QUESTIONARIES

(*1*) *Questionary A.* Questionary A was for the purpose of obtaining general information and of giving instructions. It stated that all appointments must be kept in order to secure credit for the work, that the scores in the experiments would not affect the grade in the course provided all parts were conscientiously done, and that they should not discuss the experiment with any one taking the elementary course.

(*2*) *Questionary B.* A daily report of *S*'s physical and mental condition was secured through this questionary, which was filled out at the close of each of the first four periods. She was asked to report concerning the number of hours she had slept the

[48] *Op. cit.*, 218; cf. footnote 47. [49] *Loc. cit.*

previous night, whether sleep was continuous or broken, to note the affective and emotional quality of dreams, to state whether she had eaten breakfast and lunch on that day, and to evaluate her physical condition and 'frame of mind' according to a scale given her.

(*3*) *Questionary C.* This questionary was filled out twice, a week apart, by every *S* for herself. It was then filled out by a member of *S*'s immediate family, preferably her mother, and by three young women friends. (Through misunderstanding 6 of the 132 in the last group were filled out by men friends; this was not discovered in time to correct the error.) The intra-group method of procuring judgments could not be used, since the *S*s were for the most part strangers to each other. The following instructions were printed on the first sheet of the questionary:

"Please fill out this questionary carefully and frankly. It will be considered absolutely confidential and used only in connection with the research problem of which it is a part.

A number of characteristics have been arranged in pairs: 1, (a) and (b); 2, (a) and (b), etc. Each pair is composed of two opposed traits. To fill out the questionary you are to assign a group number to each characteristic (a) and to each characteristic (b). First imagine all the young women *of the population at large* who are between the ages of 17 and 25 years and who have had at least average educational advantages, that is, at least a year or two of high school work, divided into six groups according to the degree to which they possess each of the characteristics (a) and each of the characteristics (b). Those in group 1 are the one sixth of the entire section of the population selected for comparison (chosen on a basis of age and education) who have the characteristic under consideration, for instance 1 (a), to the least degree. Those in group 2 are the sixth having it to a greater degree and so on. Group 6 will be the sixth who have it to the greatest degree. The same will be true when trait 1 (b) is considered, except that the grouping will be reversed, since the traits are arranged in pairs of opposed characteristics; those who were in group 1 on a basis of trait 1 (a) will be in group 6 on a basis of trait 1 (b) and those in group 4 for characteristic 1 (a) will be in group 3 for 1 (b). To assign a group number to the characteristics listed below, decide in which of the six groups the person for whom you are filling out the questionary would belong with respect to each of the traits, first, 1 (a), and then 1 (b), and place the numbers of the groups in the appropriate places; next do the same for 2 (a), then for 2 (b) and so on. *Do not omit any.* The *sum* of the two group numbers assigned for any pair *must be seven.* Do not place the person between groups. You must place her in one of the six groups. Read the entire list through once before you begin to assign group numbers. Be careful to distinguish between characteristics which at first appear to be identical; no two are just the same."

A few of the sixty traits are listed here:

1. (a) To be quick in her movements,
   (b) to be slow in them. (a)——(b)——
4. (a) To work carefully and accurately,
   (b) to work carelessly and inaccurately. (a)——(b)——
18. (a) To be fluctuating,
   (b) to be resolute. (a)——(b)——
26. (a) To be gloomy and depressed,
   (b) to be cheerful and happy. (a)——(b)——

31. (a) To become angry easily, to be quick tempered,
(b) to be slow to anger and only under great stress. (a)——(b)——
47. (a) To act after deliberation,
(b) to act impulsively. (a)——(b)——

The method of scoring was to consider separately the traits, *i.e.* either (a) or (b), which were thought to indicate quickness, strength, activity, steadiness, or a tendency to be cheerful. We shall speak of these as positive traits. There are 14 traits considered indicative of quickness, 29 which were thought to be suggestive of strength, 6 were taken as indicative of activity and 7 to ndicate steadiness. Seven characteristics were thought to point to a tendency to be cheerful; ease of, or quick initial arousal of, the emotions seemed to be suggested by 6; and an idea of the intensity or strength of the emotions may be gained from 4 traits.

For the final score for a trait the 2 numbers assigned by *S* to a trait were averaged and this score averaged with the 4 numbers assigned by the other persons to the same trait. With the scale of rating used, the greatest possible range for the 44 scores for any one trait is 5. The least actual range for any one of the 60 traits was 1.9, the maximum 5, and the average range 3.5. To find the composite rate and strength scores for the questionary, we found the average score for each of the positive traits, then gave as a rate score for *S* the percentage of times, out of the 14 possibilities, that she gained a score above the average for a trait in the rate group, and as a strength score the percentage of times she secured a rating above average in the 29 strength traits. The composite scores for the remaining groups, where the number of traits included was smaller, were the total number of points gained in a group by receiving a score of +1 for each trait within the group for which *S* secured a rating above the average for the trait and −1 for each trait in which she was below average. A few of these scores are given in Table VII.

TABLE VII
Questionary C Scores

S	Rate	Strength	Steadiness	Activity				Emotions		Mood	
				Physical	Mental	Both P&M	Total	Arousal Rate	Intensity	Score	Steadiness
A	89	69	+1	0	+3	+1	+4	+6	+4	+4	−1
B	89	55	+1	0	+3	+1	+4	+4	0	−1	−1
F	7	48	+3	−2	−1	−1	−4	−4	0	+4	−1
M	64	86	+5	0	−1	+1	0	−4	+2	0	+1

(*4*) *Questionary D.* *S* filled out this questionary after completing Questionary C. Part A consisted of a list of 35 activities such as baseball, dramatics, designing or drawing, and cooking. *S* was instructed to consider herself a participant in these and assign a value of −1 if displeasure or unpleasantness was derived from an activity, of 0 if neither displeasure nor enjoyment was derived, and a value of 1, 2, 3, or 4 as the degree of pleasure increased. Part B inquired into *S*'s habits of sleep; information concerning past and present health conditions was sought in parts C, D, and F. The questions in part E pertained to nervous habits such as the biting of the finger-nails and rocking. We made use of these data in the case studies which follow the statistical treatment of the results obtained from the experiments and the preceding set of questions.

III. Interrelation of Results

In Tables VIII and IX will be found the coefficients of correlation between the various experimental scores and between each score and the composite score for the series. The second composite rate score used in Table VIII was computed on a basis of the scores which had both a coefficient of reliability of .700[50] or more and a coefficient of correlation with the first composite score of at least .270. The second composite strength score in Table IX was based on the scores which had a coefficient of reliability of .429 or higher and a coefficient of correlation with the first composite score of at least .292. The coefficient of correlation between the two composite rate scores and between the two composite strength scores, however, are .906 ±.018 and .908 ±.009 respectively; also the coefficients of correlation between each one of the selected experiments and the second composite score were not very different from those obtained for the same experiment with the first composite scores. The 8 rate experiments gave an average coefficient of correlation of .426 with the first composite score and of .448 with the second composite score; the 7 strength experiments gave an average coefficient of correlation of .418 with the first composite score and of .454 with the second composite score. For these reasons we have used, to any extent, only the composite scores based on the entire series of experiments.

The coefficients of correlation between the 14 rate experiments can scarcely be said to give evidence of any one factor running through all. Fifty-eight of the 91 coefficients for pairs of experiments are positive; only 4 are at least 5 times their P.E., and 2 are 4 times it. The coefficients between the separate scores and the composite score are higher for the most part; 13 of the 14 are positive, 5 of these are 5 times the P.E. and one is 4 times it. The correlations are highest between the experiments in the group where the time was purely voluntary. The Münsterberg and cancellation experiments where speed and accuracy were stressed equally correlate relatively well with each other, and with the voluntary times for reading a selection and for judging the weights. The experiments requiring maximum speed do not here correlate well either with each other or with those of the other two types just mentioned. A similar impression is gained from an inspection of the coefficients for the strength scores. Of the 45 coefficients of correlation be-

[50] Probable errors for coefficients of correlation where N = 44 are:

r	P.E.	r	P.E.	r	P.E.	r	P.E.
.000	.102	.250	.095	.500	.076	.750	.044
.050	.101	.300	.093	.550	.071	.800	.037
.100	.101	.350	.089	.600	.065	.850	.028
.150	.099	.400	.085	.650	.059	.900	.019
.200	.098	.450	.081	.700	.052	.950	.009

TABLE VIII

Coefficients of Correlation between Rate Scores

	R−1 Tap	R−2 S.R.T	R−3 A.R.T	R−4 N.Syls	R−5 CC.S	R−6 Maze	R−7 Mun.	R−8 Can.	R−9 Cl.P.	R−10 Myst.	R−11 R.T	R−12 J.Wt.	R−13 CC.V	R−14 CC S/V	Pulse Rate	Temp.
R−1	+.878															
R−2	+.236	+.689														
R−3	+.022	−.020	+.698													
R−4	−.242	−.094	−.198	+.663												
R−5	+.059	−.211	+.098	+.096	+.897											
R−6	+.175	−.126	+.018	−.204	+.224	+.809										
R−7	+.112	+.071	+.010	+.212	−.020	+.196	+.882									
R−8	+.229	+.103	+.039	+.303	−.091	+.115	+.495	+.883								
R−9	+.120	+.041	+.329	+.150	−.041	+.246	+.181	+.454	+.794							
R−10	−.185	−.221	−.099	−.071	+.144	+.004	+.003	−.118	+.021	+.628						
R−11	+.327	−.038	−.238	+.181	+.121	+.055	+.305	+.385	−.033	+.024	+.852					
R−12	−.006	−.231	+.224	+.213	+.279	+.057	+.358	+.314	−.026	−.043	+.284	+.711				
R−13	+.041	−.130	+.016	+.031	+.715	−.037	−.053	−.025	−.153	+.273	+.297	+.236	+.813			
R−14	−.187	−.006	+.033	+.176	−.113	−.270	−.140	+.047	−.140	+.234	+.212	+.069	+.558	+.688		
P.R.	+.127	−.162	+.157	−.452	−.032	−.189	+.066	−.067	−.300	+.059	−.055	+.273	−.075	−.143	+.792	
Comp. Sc. 1.	+.094	−.03	+.239	+.293	+.277	+.271	+.419	+.587	+.366	+.144	+.455	+.581	+.455	+.275	+.098	−.0096
Comp. Sc. 2.					+.405	+.340	+.418	+.571	+.316		+.441	+.621	+.469			
Av. r. N=13	+.054	−.048	+.018	+.043	+.097	+.035	+.133	+.173	+.088	−.003	+.145	+.133	+.136	−.036		

TABLE IX

Coefficients of Correlation between Strength Scores

	S−1 St/Wt	S−2 C.C.	S−3 W's A.	S−4 Maze	S−5 Mûn.	S−6 Can.	S−7 Cl. P.	S−8 Myst.	S−9 R. Id.	S−10 Sug.	Blood Pres.	Temp
S−1	+.*749*											
S−2	+.003	+.*833*										
S−3	+.162	−.142	+.*425*									
S−4	−.163	+.070	−.217	+.*429*								
S−5	−.201	+.222	+.124	+.159	+.*869*							
S−6	+.205	−.062	+.028	+.424	+.264	+.*800*						
S−7	+.111	+.051	+.130	+.173	−.015	+.246	+.*638*					
S−8	−.268	+.176	−.140	+.25	+.071	+.322	+.291	+.*503*				
S−9	−.103	−.083	−.043	−.187	−.101	+.261	+.006	+.126	+.*764*			
S−10	−.048	+.007	−.009	+.034	+.01	+.364	+.22	+.332	+.384	+.*667*		
Bl. P.	+.179	+.009	−.016	−.302	+.018	+.227	−.113	−.187	+.247	+.362	+.*716*	
Comp. Sc. 1.	−.275	+.231	−.083	+.432	+.343	+.552	+.38	+.47	+.292	+.46	+.058	−.129
Comp. Sc. 2				+.412	+.302	+.652	+.416	+.584	+.248	+.565		
Av. r. N = 9	−.034	+.045	−.012	+.06	+.059	+.228	+.135	+.127	+.029	+.145		

tween the 10 experiments, 30 are positive, but only 1 is 5 times its P.E. and 2 are 4 times it. When correlated with the composite score, 4 of the 10 are at least 5 times the P.E. and 1 is 4 times it. Here again the experiments in which speed and accuracy are stressed equally appear to have some degree of interrelation; also the coefficient of correlation between the test for resistance to suggestion, where the rate factor did not enter

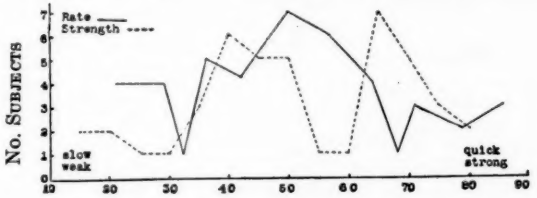


Fig. I. Frequency Curves for Composite Experimental Rate and Strength Scores. Scores by Percent Method

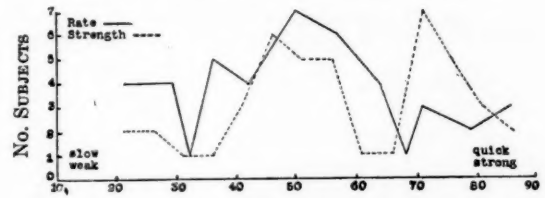
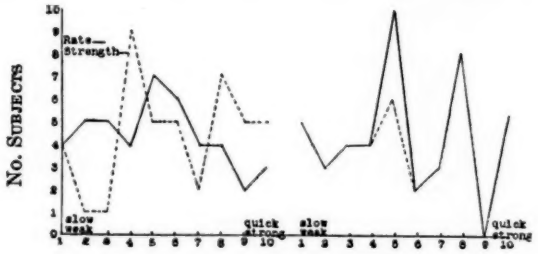


Fig. II. Frequency Curves for Composite Experimental Rate and Strength Scores with Strength Curve shifted so that the Ranges Coincide

at all, and the experiments in which quality rather than speed element predominated, suggest a closer degree of interrelation than do most of the other coefficients.

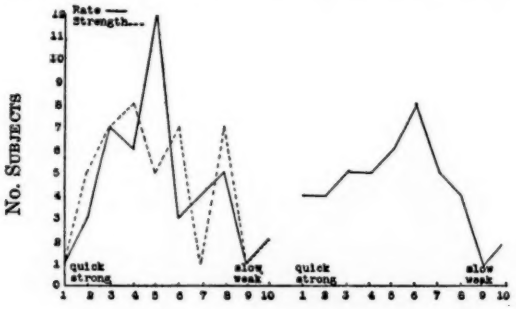
If we assume that a common rate factor and a common strength factor may exist, the question then arises whether there will be found any grouping of the *S*s with respect to these traits. Frequency curves for the composite rate, composite strength, and a combined rate and strength score are given in Figs. I, II, III, and IV. Those in Figs. I and II are plotted from the actual composite scores, whereas in all later curves the range of scores has been divided into 10 and the scores grouped accordingly. The range for the strength scores is just 6 points lower on the scale of 0–100%, therefore the distributions given in Fig. I

were plotted in Fig. II with the strength scores all shifted 6 points up on the scale, so that the position of the high and low points of the two curves might be compared. The strength curve is unquestionaby bi-modal; the rate curve can scarcely be so considered, though it does rise to a lesser extent where peaks are found in the strength curve and falls to points equally low at the lowest places in the strength curve. The agreement be-



(A) Scores (B) Scores

Fig. III. A. Composite Experimental Rate and Strength Scores (Percent Method). B. Combined Scores, Rate and Strength



(A) Scores (B) Scores

Fig. IV. A. Composite Experimental Rate and Strength Scores (Av. R. Method). B. Combined Scores, Rate and Strength

tween the curves is sufficient to suggest that a single factor might be influencing both the rate and strength aspects of the experiments. There is no evidence of this however in the coefficient of correlation for these two composite scores; it proves to be $-.0025$. Fig. V is a graphic representation of this zero correlation.

In Fig. III-A the scores plotted are the same as those in Figs. I and II. They are, however, plotted as they fall into the 10 groups into which the entire range has been divided. The nature of the curves remains unchanged. For Fig. III-B each *S*'s rate and strength scores (percent method) were added and the total plotted. Curve B is definitely bi-modal. There appears to be a quick-strong group, and another group which a study of the 10 cases falling on the fifth abscissa shows to be

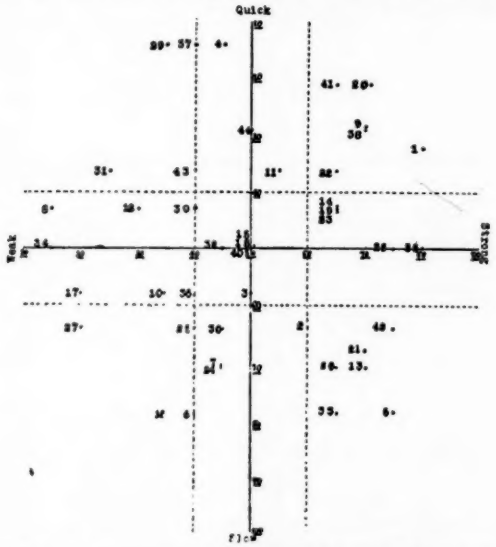


FIG. V. GRAPHIC REPRESENTATION OF THE LACK OF CORRELATION BETWEEN RATE AND STRENGTH SCORES

composed of 6 *S*s with a low rate score and a high strength score that would classify them as slow-strong and 4 cases whose scores were both close to 50%. The line dotted between abscissae 4 and 6 indicates where the slow-strong group alone would fall. There are no quick-weak *S*s in this group. There is the suggestion in this curve of a possible 3rd group of slow-weak individuals.

The scores plotted in Fig. IV, A and B, are the composite scores found by the average rank method, and are plotted as in Fig. III. The separate rate and strength curves have much the same form, but the curve for the combined scores loses its bi-modality. As has been said before, we do not consider this

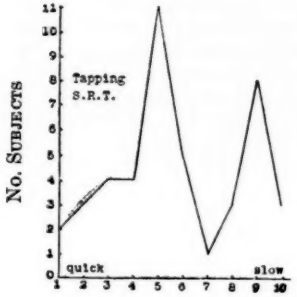


FIG. VI. COMPOSITE SCORE FOR EXPERIMENTS WHERE RATE IS ONLY FACTOR

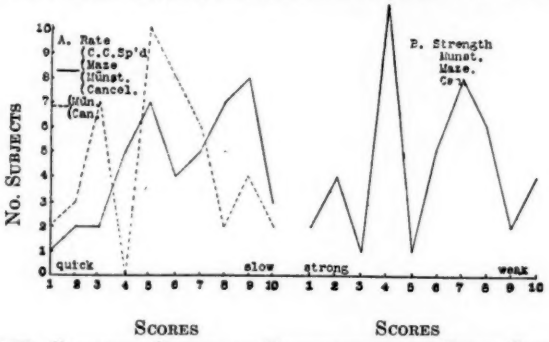


FIG. VII. COMPOSITE SCORES FOR EXPERIMENTS WITH EQUAL STRESS ON SPEED AND ACCURACY

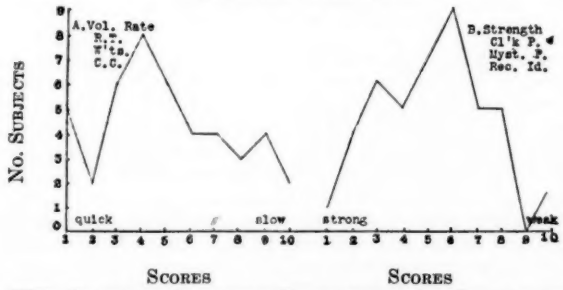


FIG. VIII. COMPOSITE SCORES FOR EXPERIMENTS WITH STRESS EITHER ON RATE OR ON QUALITY

method of computing the composite scores as satisfactory as the percentage method, since undue weight may be given to an extreme score opposed to the general tendency of *S*'s performances, when this reversal may have been caused by a sudden shift in attitude due to extraneous factors.

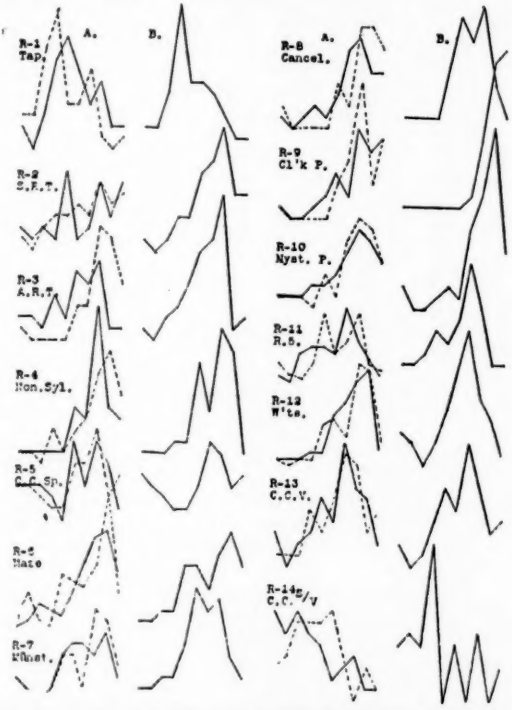


FIG. IX. FREQUENCY CURVES FOR THREE SETS OF SCORES FOR EVERY RATE EXPERIMENT

We next broke up the composite scores into scores for the various types of rate and strength experiments. The average rank method was used to obtain these scores, since 4 was the largest number of experiments in any group. These frequency curves are given in Figs. VI, VII, A and B, and VIII, A and B. The curves both for rate and for strength in the experiments requiring an equal effort for speed and accuracy are unquestionably bi-modal; as is also the curve for the average score for

tapping and simple reaction time. Bi-modality is much less marked in the curve for the experiments in which stress was laid on speed and is not present in the one for those where emphasis was on quality.

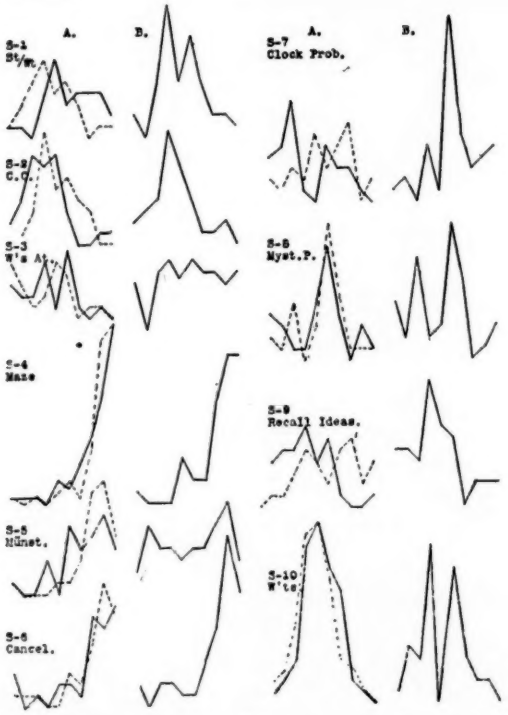


FIG. X. FREQUENCY CURVES FOR THREE SETS OF SCORES FOR EVERY STRENGTH EXPERIMENT

The search for bi-modality was carried further. We had three sets of scores for every experiment, the two sets from which the coefficients of reliability were computed and the final data. Curves were plotted for these 72 sets of scores to determine whether any experiment consistently yielded this form of curve. These curves are to be found in Figs. IX and X. The double curves in the A columns are from the scores obtained for the coefficients and those in the B columns from the final data. Of the 14 sets of rate curves the ones for *R-5* (carbon-copy speeded

time) and for *R-14* (ratio of carbon-copy speeded to carbon-copy voluntary time) are the only ones which give even the slightest indication of bi-modality in all 3 curves for any experiment. Inspection of the 10 sets of strength curves yield similar results. The set for *S-8* (mystery problem) is the only one where more than one mode can be considered to exist in all of the curves.

The results which have been studied in the form of coefficients of correlation and in frequency curves cannot be said to show definitely that there is neither a common rate nor a common strength factor. On the other hand they do not by any means indicate conclusively that such factors exist; they suggest rather that there may be such a possibility.

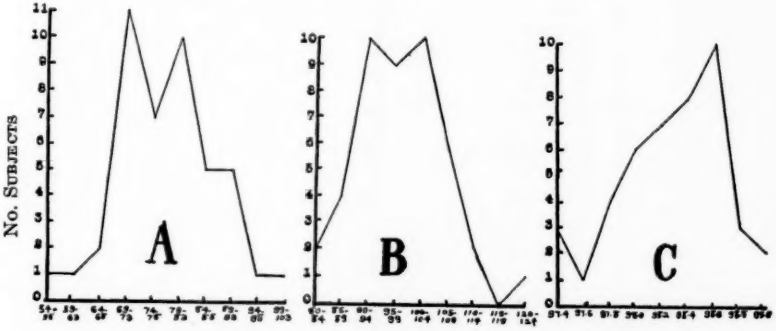


Fig. XI. Frequency Curves for Physiological Measures: A. Pulse Rate; B. Blood Pressure; C. Temperature

Curves for the physiological measures are given in Fig. XI. Neither the blood pressure nor the temperature curves give any indication of types. The curve for the pulse rate measures may be considered bi-modal and suggestive of a group of individuals with an unusually slow pulse in addition to the group whose rate agrees with what is accepted as normal. When the pulse rate for the cases scoring highest or lowest in the rate experiments is studied an unexpected relation appears to be present in spite of a coefficient of correlation of .098. Seven of the 8 *Ss* receiving the highest experimental rate scores have an average pulse rate below 79, which is the average for all 44 cases, and 24 of the 31 measures for these *Ss* are below the average, while 5 of the 8 with the lowest rate scores have an average pulse rate above 79 and 21 of the 32 measures are above it. It may be that the high pulse rate in these cases is an indication of an excited

condition which interfered with success in the experiments, especially with respect to speed. On this point it is interesting to note that the coefficient of correlation between pulse rate and rate of emotional arousal is .303; this is not high, but it is suggestive. The only *S* (BeC) who has a high pulse rate with a high experimental rate score does fall down in her strength score. Among the 5 with high pulse rate but low experimental rate scores, 4 also have low strength scores; of the 3 who have low pulse rates and low experimental rate scores, all did well in the matter of strength. A similar study with respect to blood pressure and temperature measures, and experimental scores, does not disclose any hint of a connection between them.

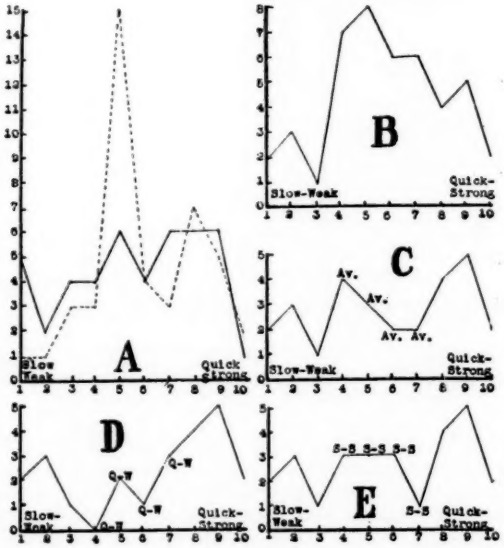


FIG. XII. QUESTIONARY: COMPOSITE RATE AND STRENGTH SCORES

A. Rate and Strength scores given separately.
B. Rate and Strength scores combined.
C. Average cases.
D. Quick-Weak cases.
E. Slow-Strong cases.

C, D, and E are plotted from the scores used in B. In them the modal group has been broken up into Quick-Weak and Slow-Strong cases and cases average in both traits. The rest of the curve remains unchanged. The *abscissae* represent the *scores*, the *ordinates* the *number of subjects*.

The frequency distribution of the composite rate scores and composite strength scores obtained from the questionaries is given in Fig. XII. The curves in part A are for these two sets of scores. Again the strength curve is more definitely bi-modal than the rate curve; the latter has 3 peaks of nearly equal height, suggesting a slow, an average, and a quick group. When the two composite scores are combined we have curve B which indicates a small slow-weak group, a larger quick-strong group, and a still larger middle group composed of the quick-weak and slow-strong cases and those average in both traits. The cases in this center group were sorted out and the next 3 curves plotted to show their distribution. In curve C only the cases average in both traits appear, in D the quick-weak, and in E the slow-strong cases; all are to some extent tri-modal. The coefficient of correlation between the composite rate and strength scores is $-.107$.

The degree of correlation existing between the other tendencies investigated by the questionary is as follows:

Steadiness and Composite Strength Score (Quest.)	$= -.557$
Activity and Composite Rate Score (Quest.)	$= +.626$
Emotional Intensity and Composite Strength Score (Quest.)	$= +.192$
Emotional Arousal (Speed) and Composite Rate (Quest.)	$= +.788$

When we come to investigate what relation exists between the experimental, physiological, and questionary results, we find that for the most part the correlations are negligible. See Tables VIII and IX for most of the correlations between physiological measures and experimental scores. Others, and those between the physiological measures and questionary results, are given below:

Blood Pressure Range on First Day and Comp. Exp. Rate Score	$= +.275$
Blood Pressure Range on First Day and Comp. Quest. Rate Score	$= +.164$
Pulse Rate and Comp. Quest. Rate score	$= -.048$
Average Blood Pressure Scores and Comp. Quest. Strength Score	$= +.019$
Pulse Rate and Rate of Emotional Arousal	$= +.303$

To make a statistical study of the relation between questionary measures of rate and strength and the experimental scores, we wished to obtain as many correlation coefficients as possible between measurements secured by these two methods. We selected first the combinations most likely to give a correlation, next we used the experiments with the highest coefficients of reliability. When 232 of the possible 555 combinations had been plotted and partially worked out and 54 of the 119 rate and 35 of the strength coefficients had been found, the results

were such that it did not seem profitable to go further with this undertaking. The extreme coefficients for the rate and strength groups as far as they were completed are as follows:

Quest. 2. (Speaks rapidly) and *R-11* (Reading Time)..... = +.355
Quest. 22 (Quick to note changes) and *R-10* (Myst. Prob.)... = +.280
Quest. 3 (Works rapidly) and *R-1* (Tapping)..... = +.256
Quest. 13 (Quick in mental processes) and *R-3* (Assoc. R. T.). = +.228
Quest. 13 (Quick in mental processes) and *R-9* (Clock Prob.). = −.250
Quest. 2 (Speaks rapidly) and *R-9* (Clock Prob.)..... = −.245
Quest. 2 (Speaks rapidly) and *R-3* (Assoc. R. T.)..... = −.242
Quest. 47 (Acts impulsively) and *R-6* (Maze)..... = −.210
Quest. 56 (Self-confident) and Comp. Exp. Strength Score.... = +.320
Quest. 37 (Intense likes and dislikes) and *S-2* (Carbon-Copy). = +.262
Quest. 10 (Dominates conversation) and *S-2* (Carbon-Copy).. = +.262
Quest. 4 (Works accurately) and *S-7* (Clock Prob.)..... = +.262
Quest. 41 (Calm in emergencies) and *S-7* (Clock Prob.)..... = +.252
Quest. 46 (Perseveres in difficulties) and Comp. Exp. Strength Score..... = +.236
Quest. 23 (Notices details) and Comp. Exp. Strength Score.... = −.223
Quest. 34 (Exerts self-control in emotion) and Comp. Exp. Strength Score..... = −.219
Quest. 15 (Appreciates humorous sayings) and Comp. Exp. Strength Score..... = −.207

Other coefficients found between composite experimental or questionary scores and scores for groups of experiments or groups of questions are given below:

Comp. Rate Score, 14 exps., and Comp. Quest. Rate Score..... = +.061
Comp. Rate Score, 8 exps., and Comp. Quest. Rate Score..... = +.052
Comp. Strength, 10 exps., and Comp. Quest. Strength Score.... = +.022
Comp. Strength Score, 7 exps., and Comp. Quest. Strength Score. = +.041
Comp. Quest. Rate Score and Tapping (speed only)..... = +.086
Comp. Quest. Rate Score and Av. R. in Tapping and Simple Reaction Time (speed only)..... = +.079
Comp. Quest. Rate Score and Av. R. in Carbon-Copy Sp'd, Maze, Münst., and Cancel. (speed and accuracy)..... = −.114
Comp. Quest. Rate Score and Av. R. in Münst. and Cancel. (speed and accuracy)..... = −.040
Comp. Quest. Rate Score and Av. R. in Reading Time, Judg.. W'ts. Carbon-Copy Vol. (voluntary rate)..... = +.227
Steadiness and Comp. Exp. Strength Score..... = +.077
Activity Score and Comp. Exp. Rate Score ..... = +.092
Emotional Intensity and Comp. Exp. Strength Score..... = −.207
Emotional Intensity and Comp. Exp. Rate Score..... = −.068

The negligible degree of correlation, in the case of both rate and strength, between experimental and questionary scores may be expressed differently. We find that 7 cases receive the same classification for both traits by the experimental and questionary scores; in 12 instances the classification is not changed to the opposite one for either trait, but one or more of the four scores are just average; in 18 cases either the rate or strength classification by one set of results is opposed to the classification by the other set, and in 7 cases both rate and strength classifications by one set are contradicted by the other.

The statistical treatment of the interrelation of results was supplemented by a study of the two extreme cases from each of the four groups, based on the experimental score. The cases are numbers 1 and 20 from the quick-strong group, 29 and 37 from the quick-weak, 5 and 23 from the slow-strong, and 16 and 27 from the slow-weak group. (See Fig. V for the grouping of the cases.) In this study we sometimes find a striking similarity between experimental results and parts of the questionary results, but equally often a marked dissimilarity, and at no time is the agreement perfect. The fact that of the 16 cases rated as definitely weak by the questionary scores only 4 are also rated slow, and that only 4 of the 21 whose scores classify them as slow are also considered definitely weak, suggests that those who filled out the questionaries may have been loath to give low ratings for both types of traits, but tended rather to balance them. Four of the case studies, one from every group, are given in illustration:

Case 1. A. An experimental rate score of 68, an experimental strength score of 80, a questionary rate score of 89 and a questionary strength score of 69 give this *S* a classification of quick-strong on a basis of both sets of results. In Questionary C she receives a rank above the average rank of the 44 cases in each of the rate traits except for rate of talking, which is just average. In the 29 traits taken as suggestive of strength her rank is above the average in 20 instances; in the other 9 she is very close to the average except in the control of her emotions and calmness in emergencies. She appears to be quick physically, mentally, and emotionally, to be forceful, energetic, and dominating, to be a steady worker, resolute and fixed in her opinions, but to be uneven in mood or disposition and to have changing antipathies and sympathies. She is ranked approximately average for optimism but below average for good-naturedness and slightly below in tendency to praise. This is suggestive of the unpleasant feeling tone which Ebbinghaus, Kreibig, and Meumann[51] among others attribute to this temperament. Another indication of this feeling quality is found when we consider the values assigned to the 35 activities listed in Questionary D; these values indicate the amount of pleasure derived from participation in the activities. To 5 activities she assigns the value which is average for the 44 *S*s, to 12 she assigns a value above the average, but one below average to 18 of them. From 11 of these 18 she derives no pleasure at all; even displeasure from 8 of them. Contrary to what we should expect of persons in this group, the data here for the most part show a disinclination for activities involving much physical exercise, also distaste for competitive games either physical or mental. This may be partly explained by a record at the University Health Department which states that her home physician advised against gymnasium because of a heart condition; this weakness may be a factor in her dislike of exercise. However, we find that in Questionary C her total score for the tendency to be active is +4, where the range of scores for all 44 cases is −6 to +6 and both average and median is 0. When we consider the next part of Questionary D, we find that *S* reports that she is a sound sleeper, that the number of nights during which she dreams is 25% where the average for all *S*s is 18.6%, but that she dreams only 5% of the average night while the average for the 36 *S*s who report any dreaming is 34.5%. She estimates her pleasant dreams at 50%

[51] W. Stern, *op cit.*, 483 f.

and her unpleasant at 25%. In our search further for evidence of nervousness we find little; she states that she very seldom bites her nails now although she often did between the ages of 4 and 12 years, that she seldom rocks while in a rocking chair, but does much of the time swing her free foot when sitting with her knees crossed. There appears in general to be fairly good agreement throughout the data on this case.

Case 29. M. Experimental scores of 86 and 35 classify *M* as quick-weak; questionary scores of 64 and 86 change the classification to quick-strong. Though the ratings are not usually very much above the average for the trait, she is considered quick in most respects; exceptions are slowness in speech and slowness to anger and impatience. When we consider the strength questions, we find that the only weak points stressed are the ease with which she is persuaded and the mild intensity of her anger. Most of the traits in this group are rated unusually high and picture her as an accurate and steady worker, energetic, industrious, resolute, self-controlled, calm in emergencies, self-confident, enthusiastic, and given to intense likes and dislikes. Optimism and a pleasant feeling tone are assigned to the quick-weak temperament, and this *S* is rated high in them. This characteristic is suggested also by her record in Questionary D, for she gives a value above average to 25 and one below average to only 4 of the activities; she derives the greatest amount of pleasure from 16 activities, and no pleasure at all, or in this case displeasure, from only one. She enjoys both physical and mental activity. *M* reports that she is a light sleeper, dreams about 75% of every night and that half her dreams are pleasant, the other half being equally divided between neutral and unpleasant. Though she often bites her nails she seldom rocks and never swings her foot. Her record at the University Health Department contains a report of "nervous trouble, cried, little things bothered." In the daily questionary she made notes on the first and fourth days that she was "nervous," but her low strength scores were not confined to those days. The data on this case are throughout not in harmony; the ratings on the strength traits are opposed to the evidence gained from all other sources.

Case 33. P. Slow-strong is the classification earned by experimental scores of 21 and 65 and slow-average by questionary scores of 29 and 48. Only in talking and working is *P* considered really quick. She is thought slow in her movements and mental processes, slow to anger and in the arousal of other emotions. The outstanding strength values indicate that she is a steady worker, resolute and courageous, and exerts great self-control in showing her emotions, but that she is inclined to be mentally indolent and inaccurate in her conclusions and opinions, to become only mildly angry and to be indifferent toward people and things. Instead of the tendency toward pessimism and unpleasant feeling tone expected of persons in this group, we find here an inclination to be cheerful and happy, to praise and be optimistic, although in contradiction to this she is also rated as habitually anxious and worried. The values assigned by her to the activities do not give evidence of a definite tendency in either direction; she gives a value above average to 15 but one below average to 13, derives the greatest pleasure from 8, but no pleasure at all, and in one instance displeasure, from 7. She claims to be a sound sleeper, to dream during few nights (5%), and not to any great extent (10%) at those times. Her dreams are chiefly neutral and only 5% are unpleasant. She also seems to be troubled little by nervous habits, for she never bites her nails, though she often did when about 10 years of age; she rocks little of the time that she is seated in a rocking chair, and seldom swings her free foot when her knees are crossed. Again, the data from various sources do not run parallel throughout.

Case 16. F. An experimental rate score of 21 and experimental strength score of 35 classify *F* as slow-weak. Her questionary rate score of 7 agrees with this; her questionary strength score of 48 places her with the average group. Most of the values for the rate traits are well below the average and those for the strength traits close to it. She is considered slow in every way, careful, accurate and steady in work; to feel losses for a long time, to be resolute, perserveing and self-controlled, but to lack force and energy, to have changing likes and dislikes, to be easily persuaded and wanting in self-confidence. We should expect persons in this group to give evidence of pleasant feeling tone. This is true in her case for she is described as good natured, cheerful and happy though inclined to alternate in mood; as given to praising and laughter, and as optimistic. There is further evidence of this tendency in the values assigned to the activities, for she gives a better than average score to 15 and a score below average to only 7. To 11 of the 15 she gives the highest possible score, while she claims to derive no pleasure or even displeasure from only 3. Activities involving either physical or mental exertion are scored high, although her total score for activity in Questionary C is −4; the latter is more in keeping with what one would expect of a person classified as weak and slow. *F* reports that she sleeps soundly, dreams during about one night in 4 and only about 25% of those nights; 50% of her dreams are pleasant and 25% neutral. She apparently does not have any of the common habits suggestive of nervousness, for she very seldom bites her nails, seldom rocks, and very seldom swings her foot. The conservative judgments concerning most of the strength traits weaken an otherwise close agreement between the various sets of data.

IV. Conclusions

(1) There is a slight indication of the possibility of a common rate factor in the experiments in which speed and accuracy were equally stressed and in those where the rate was voluntary.

(2) A common strength factor is suggested to a slight degree by the results of the experiments in which speed and accuracy were stressed equally and in those where the emphasis was on the quality of the performance.

(3) The 44 cases do not fall into four sharply defined groups on a basis of the experimental scores, but at the same time there do seem to be a few cases who are consistently quick and are either consistently strong or consistently weak, and others consistently slow, who are fairly consistent in their strength or weakness.

(4) The physical measures give practically no evidence of types and do not in general appear to be consistent with the experimental results. An exception to this is the negative relationship between the extreme experimental rate scores, both high and low, and pulse rate. We suggested above that this may be indicative of an excited state which interfered with the performance of the experiments.

(5) The questionary rate and strength scores also do not divide the *S*s into distinct groups, but again, as with the experi-

mental scores, there appear to be some cases who are consistent in the matter of both rate and strength.

(6) The agreement between the experimental and questionary classifications is slight enough to be accounted for by chance.

(7) In general we must conclude that although there may be general rate and strength factors as we have defined them, still, in so far as the questionaries yield evidence concerning temperament, these factors do not seem to be related to temperament.

THE UPPER LIMIT OF AUDITORY LOCALIZATION

By H. M. HALVERSON, Yale University

Investigators in sound localization agree that high tones, above 800 d.v., are localized with difficulty; *i.e.* that accuracy in judgments of position of a tone decreases with increase in its frequency.[1] There is also general agreement that the tones move over a more limited space with differences of phase.[2] There is, however, little agreement as to the frequency at which the lateral movement of the tone ceases to function.[3]

HISTORY OF THE PROBLEM

Rayleigh[4] (1907) got good right and left displacements of the tonal image with a 512 d.v. tone, fair lateral shifts with a 640 d.v. tone, and rather poor results with a 768 d.v. tone. Wilson and Myers[5] (1908) using the König steel bars were unable to obtain changes in localization in the case of high tones. Bowlker[6] (1908) found for frequencies of 340 d.v. and less that lateral displacements of the tone were as great as 90°. The lateral displacement of a 490 d.v. tone, based on his figures for the so-called "cross-over"[7] angle, was 42.5°. For frequencies greater than approximately 700 d.v. the maximal displacement was "roughly proportional to the wave-length." At 2310 d.v. the displacement was 7°, while at 3100 d.v. he believed the maximal displacement to be 5-6° (not observed). The law broke down for frequencies greater than 3100 d.v.; the displacement for 6770 d.v., for example, was greater than the law would allow. Bowlker's results are shown in Fig. 1. More[8] (1909) stated that 512 d.v. was about his limit for accuracy in localization, that for higher frequencies there was a decrease in accuracy, and that at 1024 d.v. his judgments were not reliable. Brief trials convinced More of the impossibility of localizing direction with a 3000 d.v. tone. Stewart[9] (1920) probably obtained movements at 1280 d.v., but none at 1536 d.v. Hornbostel and Wertheimer (1920)[10] stated that with tones higher than about 800 d.v. the lateral movement no longer extended to 90° right and left but decreased with the frequency. At about 17,000 d.v. the lateral movement should be reduced to nil, *i.e.* the tone should be heard in the median plane of the head. Banister[11] (1925) secured movements for 1040 d.v. and 1345 d.v., although the results were of anomolous character. In substance he agreed with Stewart[12] that movements of the tone occurred for frequencies up to about 1400 d.v.

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[1]See Rayleigh, More, Wilson and Myers, Bowlker, Stewert, Banister, and Boring, referred to below.

[2]Banister takes exception to this rule. He doubts the validity of both Bowlker's and Stewart's conclusions that greater lateral movements of the tone occur for lower frequencies than for higher frequencies. H. Banister, *Brit. J. Psychol.*, 15, 1925, 305.

[3]E. G. Boring, this JOURNAL, 37, 1926, 173.

[4]Lord Rayleigh, *Phil. Mag.*, 6 ser., 13, 1907, 228-229.

[5]H. A. Wilson and C. S. Myers, *Brit. J. Psychol.*, 2, 1908, 368.

[6]T. J. Bowlker, *Phil. Mag.*, 6 ser, 15 ,1908, 323-327.

[7]Bowlker, *op.cit.*, 323.

[8]L. T. More, *Phil. Mag.*, 6 ser., 18, 1909, 314.

[9]G. W. Stewart, *Phys. Rev.*, 2 ser., 15, 1920, 437.

[10]E. M. von Hornbostel and M. Wertheimer, *Sitzungsber. d. preuss. Akad. d. Wissensch.*, 20, 1920, 395.

[11]Banister, *op.cit.*, 291-292.

[12]Banister, *op.cit.*, 305.

Hartley[13] (1919) plotted theoretical curves to show the angular displacement for any given difference of phase for tones of the following frequencies: 310, 620, 930, 1240, and 1860 d.v. According to these curves tones of higher frequency than about 650 should have a maximal movement of less than 90° from the median plane. At 1860 d.v. the movement should be about 27°.[14] He pointed out that his curves corresponded fairly closely with Bowlker's findings.[15]

We are then forced to the conclusion that for frequencies below 600-800 d.v. displacements may extend to the extreme lateral position.[16] Movement becomes more and more restricted for higher frequencies until we reach a point (frequency) above which the sound remains fixed in the median plane. We have seen that Stewart and Banister placed this point between 1200 and 1500 d.v. Hartley and Bowlker refused to commit themselves, but their data placed it above 7000 d.v., and Hornbostel and Wertheimer as a result of their investigations with time differences gave 17,000 d.v. as the approximate frequency at which movement ceased. It was for the purpose of throwing light on this problem that the investigation herewith reported was undertaken.

Apparatus and Procedure

The precise object of the experimentation then consisted in finding the maximal amount of lateral movement which is obtainable from each of a number of tones varying in frequency from 600 d.v. to 17,000 d.v. The method of procedure varied in accordance with the difficulty in making localization.[17] The following persons served as observers: Mr. Harold E. Pressey (P), graduate student in psychology; Mr. John M. McGinnis (M), instructor in psychology; and the writer (H). Neither P nor M had had previous training in auditory localization; H was trained.

For the first three frequencies, 600, 1000, and 1400 d.v., we used electrically driven audio-oscillator forks.[18] The fork was put in a sound-proof box and placed in a closet. An iron T-tube, inside diameter of 3/8 in., was placed so that the stem of the T lay perpendicular to the outside face of one of the prongs. A thin piece of tin of the width of the diameter of the T-tube had been driven symmetrically into the stem so that the latter was divided into two equal parts as far back as the top of the T-tube. Finally, the inside of the T-tube was smoothed out by means of melted wax. This preparation insured against interference of sound waves from one tube to the other in the two rubber tubes (1/2 in. inside diameter) leading from the branches of the T-tube to the stethoscopic binaurals in a second room. We had marked off on a wall-board lying on a table a large semi-circumference of a radius of 70 cm. upon which were erected rectangular wooden posts, which with their appropriate numbers were placed at intervals of 5° on the semi-circumference and served as localizing cues for the *O*, who, with the binaurals in his ears was seated at the center of the circle. One of the rubber tubes in the second room was cut in two, and two glass tubes, each about 125 cm. in length and one telescopically containing the other, were inserted in the gap, so that the length of this conductor could be varied by sliding the inner tube in or out of the larger. The larger tube, with a meter stick fastened parallel to it, was secured to the table. The open end of the inner tube served to indicate the position which was to be used for observation. Eight (for 1400 d.v., seven) positions, previously selected to offer the best opportunity for observing changes in localization accompanying differences in phase, were used in the case of

[13] R. V. L. Hartley, *Phys. Rev.*, 2 ser., 13, 1919, 373-385.
[14] Hartley, *op. cit.*, 380. [15] Hartley, *op. cit.*, 384. [16] Boring, *op. cit.*, 173.
[17] Boring, *op. cit.*, 172-173.
[18] Purchased from General Radio Co., Cambridge Mass. These forks emit a remarkably pure tone.

the three frequencies above mentioned. The observer took his position, and, with the fork sounding, the apparatus was set for one of these positions. The *O* placed the binaurals to his ears, localized the sound, removed the binaurals and gave his judgment verbally to *E*. The stimuli were presented in chance order with no repetitions. Under the scheme adopted, *O* was presented with an auditory stimulus and directed to report its position in terms of degrees right or left of the median plane of the head. It developed under practice that while the positions on the wall-board were indicated in multiples of 5's, the *O*s were content to render judgments in terms of these multiples at the extreme lateral positions, but they resorted to finer judgments whenever the localizations were within 45° of the median plane.

For frequencies above 1400 d.v. we replaced the electrically driven forks by an electro-magnetic ribbon transmitter.[19] This source of sound was a phosphor bronz ribbon under tension longitudinally in the magnetic field of a strong permanent magnet from a Weston ammeter. The transmitter was energized by a Hartley oscillator[20] the parts of which consisted of a Leeds and Northrup Brook's Inductometer with a maximum self inductance of 0.250 henry, a Leeds and Northrup three dial mica condenser with a maximum reading of 1.11 microfarads, and a Western Electric 205-B vacuum tube operated on a plate potential of 120 volts. The transmitter was put in the sound-proof box with the T-tube so situated that its stem rested immediately above the ribbon. We reduced both arms of the tube circuit leading from the T-tube to the binaurals to 40 cm. each because we found that the tubes rapidly absorbed the high tones, and replaced the glass tubes by telescopic rubber tubing (the inside diameters of which were ½ and 9/32 inches), because the shortness of the arms necessitated the use of flexible conductors. For frequencies of 10,000 d.v. and higher the conductors were reduced in length to 25 cm. For frequencies above 10,000 d.v. each arm was but 25 cm. in length. We also replaced the wall-board design by a wall schema consisting of pieces of cardboard appropriately numbered and placed on the wall before *O*. These numbers now served as the localization clues. The positions used for purposes of localization were indicated on the inner of the telescopic tubes, which had previously been marked off in half-centimeters.

For frequencies below 3500 d.v. the *O*s followed the directions outlined above in making their localizations, with the exception that they kept the binaurals in the ears in order that they might better follow the movement of the sound. This is a necessary precaution, for, with all high frequencies, two images are usually present throughout the experimentation. For frequencies of 3500 d.v. and above the *O*s with the binaurals constantly in the ears stated merely the extreme lateral displacement of the sound. This later instruction for the very high frequencies was adopted because median localizations were practically precluded; the intensity of the sound at medial positions was reduced almost to nil.

Results

Preliminary trials with each frequency showed that lengthening or shortening the telescopic arm of the apparatus an amount equal to one wave length of the frequency used produced all possible differences of phase (0°-360°).

While the main problem was not concerned with *O*'s ability to localize sound for various differences of phase, observations

[19]The transmitter, designed by Dr. A. L. Fitch, of the Dept. of Physics, University of Maine, will be described in a forthcoming number of the *Physical Review*.

[20]H. J. van der Bijl, *The thermionic vacuum tube*, 1920, 282-284.

on the eight positions used for the three lower frequencies gave the *O*'s excellent training for the experiment and served to show the reliability of judgments of position of sounds at these frequencies. The results for each *O* for the three lower frequencies are shown in Table I.

There is fair agreement among the *O*s on their localizations for the three frequencies. The mean variations and the number of judgments show that the difficulty experienced in judging direction increases with the amount of displacement of the tone from the median plane of the head.

To assure ourselves that the judgments recorded in the table are those for the image selected for experimentation we instructed the *O*s to keep the binaurals at the ears and observe the movement of the image while the telescopic tube was lengthened and shortened. The *O*s experienced no difficulty in following the passage of the image across the median plane to its extreme lateral position. It was during this procedure that the *O*s discovered that the image, observed in movement, attained more extreme lateral positions than those positions obtained by observations taken while the image was at rest. For example, the image for 600 d.v. was observed by all *O*s to describe a complete arc of 180° about the head in front. For 1000 d.v. and 1400 d.v. the arcs were respectively about 110° and 70°. Double images were of frequent occurrence and were recorded, but are not shown in the table. For frequencies above 1400 d.v. the main purpose of the experiment was to determine the maximal displacement of the sound image from the median plane of the *O*'s head for varying differences of phase of the frequencies. The *O*s were instructed to keep the binaurals at the ears (removing them only at rest periods) while the experimenter made adjustments of the tubes preceding their judgments of position. This procedure enabled the *O*s to observe the image in motion before it came to rest at the position for which the judgment was given. Inasmuch as the moving image is more easily observed than is the image at rest, the *O*s had greater confidence in the accuracy of their judgments of its position. They caught the image while it was moving, followed it until it reached its most lateral position and then reported this position. In this manner 25 judgments were taken for each side. The quantitative results are shown in Table II.

In general it may be said that as the vibration frequency increased, the tone moved through a more limited region about the median position. This decrease in the amount of lateral displacement of the tone was rapid at first and then less rapid as we approach the very high frequencies, when the amount of displacement remained fairly constant. From our observations

TABLE I

The frequencies are indicated at the top of the table. Each cell contains the localization in degrees for the several positions on the cm. scale (top row of figures) of the inner telescopic tube, the m.v. in degrees, and the number of judgments made on the image out of a total of 40 trials. For each frequency the localizations in the four left columns are all to the *O*'s left; the others are to his right.

O		600 d.v. Cm. on scale								1000 d.v. Cm. on scale								1400 d.v. Cm. on scale						
		60	51	49	40	26	15	6	1	50	45	40	35	30	25	20	15	52	48	44	40	36	32	28
P	Local: deg.	80.1	75.8	39.2	6.0	19.2	36.9	76.0	85.3	36.7	27.3	20.5	8.9	7.0	21.1	32.8	45.5	3.0	8.2	10.1	8.1	4.4	13.8	23.0
	m.v.	8.5	8.2	10.1	4.9	6.8	7.2	8.3	3.5	6.5	5.3	7.1	5.4	3.7	6.3	6.8	8.8	0.0	2.2	2.2	2.5	3.0	3.2	6.8
	No. jdgmts.	31	38	40	40	40	40	36	33	38	37	40	40	40	40	40	31	1	16	34	32	34	38	40
M	Local: deg.	67.8	57.0	55.4	23.1	8.2	33.0	54.5	45.7	48.3	26.9	17.9	4.7	8.5	26.0	33.2	41.5	14.9	16.4	12.0	3.5	6.7	14.0	19.0
	m.v	11.1	7.8	6.7	5.2	3.4	5.6	11.0	8.1	5.4	6.1	4.3	2.4	2.1	5.5	6.4	8.1	4.6	3.7	4.1	1.7	3.8	2.5	2.8
	No jdgmts.	37	40	40	40	40	40	28	30	40	40	40	40	40	40	40	13	39	40	40	40	19	26	38
H	Local: deg.	90.0	82.9	76.3	22.9	0.7	18.6	55.1	72.0	58.4	30.1	23.4	13.2	0.3	12.5	23.3	48.5	20.3	31.4	20.7	12.8	1.7	14.4	21.0
	m.v.	0.0	8.6	5.2	4.7	1.2	3.1	6.9	8.5	4.3	4.1	3.8	4.6	2.2	3.5	3.1	7.7	7.7	4.6	5.8	4.4	4.3	3.4	4.5
	No jdgmts.	36	28	33	40	40	40	39	32	40	40	40	40	40	40	40	37	21	40	39	40	38	40	34

TABLE II

The frequencies used head the columns. The cells immediately below give the maximal displacements from the median plane in degrees. The third and fourth rows of cells show respectively the average maximal displacements and the average m.v.'s for all *O*s.

O	1800	2200	3000	3500	4000	5000	6000	7000	8000	9000	10000	11000	12000	13000
P	22.5	22.5	20.0	12.3	13.4	17.0	13.2	10.5	8.7	8.4	6.4	8.6	9.2	9.5
M	22.0	17.5	17.5	17.7	14.8	15.4	10.2	9.8	3.2	3.1	5.2	5.6	4.5	4.7
H	18.5	18.5	15.0	11.8	12.9	11.1	8.9	8.1	5.9	4.2	5.0	5.0		
Av	21.0	19.5	17.5	13.9	13.7	14.5	10.8	9.5	5.9	5.2	5.5	6.4	6.8	7.1
Av. m.v.	3.2	1.5	2.1	1.5	1.3	1.3	1.2	1.5	1.2	1.2	0.8	1.0	0.9	0.9

it would appear that displacements of the tones should fail to occur only at the upper limits of audibility.

The tones for the various frequencies were remarkably pure. We made no effort to determine the number of partials present or their prominence. When the sound was very intense, one or more partials were observed, but under the conditions of the experiment in which the weakest possible tones were used the observers were able only very occasionally to detect the presence of partials. When partials were noticeably present in the sound, a slight adjustment of the ribbon was sufficient to remedy matters. In any case the partials were so weak as to preclude any possibility of confusing them with the tone in question.

Confidence in the results of the experimentation was substantially increased, first, by the fact that all *O*s experienced very little difficulty in the localization of the tones of any frequency up to the point where the sound was no longer audible through the conducting tubes. For H this point was 12,000 d.v., for P and M, 14,000 d.v. Incidentally, H's upper limit of hearing with open ears by this apparatus was found to be between 14,000 and 15,000 d.v., while the upper limits for P and M were above 18,000 d.v. (Tones of these higher frequencies resembled closely those for very high frequencies as produced by the Galton whistle.) No effort was made to find the exact upper limits of the *O*s. Moreover, all through the experiment *E* was able to obtain every possible difference of phase by lengthening or shortening the telescopic tubes a distance equal to the wavelength of the particular tone being localized.

To show that localization of tones of high frequencies was not particularly difficult, we obtained additional judgments of direction for the frequencies 1800, 2200, and 3000 d.v., such as the median position, 5L, 5R, 10L, 10R, 15L, and 15R. The results, as measured by the cm. scale on the telescopic tube for 25 judgments on each of these positions, show considerable consistency in the judgment of direction both for the individual *O*s and among the three *O*s; the mean variation for any one *O* ranged between 2.6 cm. (for 1800 d.v.) and 0.5 cm.

Fig. I shows graphically for the many frequencies used the maximal amount of angular displacement of the tone from the median plane of the head toward one side; *i.e.* 60° means that the tone will sweep through 120° in the complete phase cycle, and 10° means it will sweep through 20° in the full cycle. The isolated dots give the average angular displacements for all *O*s for the various frequencies and the continuous curve is the best fitting line of the dots. If we assume continuity of function, the successive amounts of angular displacements may be read from the ordinate scale. Thus, the angular displacement at 600 d.v., the lowest frequency used, is 76.4°, that for 1000 d.v. is 47.2°,

and so on. On the basis of the results obtained we may regard the smoothed curve as representing in general the form and amount of the function for the angular displacement of tones ranging from 600 to 14,000 d.v.

The dotted curve is the function obtained by Bowlker[21] and is based on the lateral displacements of the sound at the 'double image' point. Bowlker claimed that the displacement was greater[22] than his figures for the "double images" admit, and his

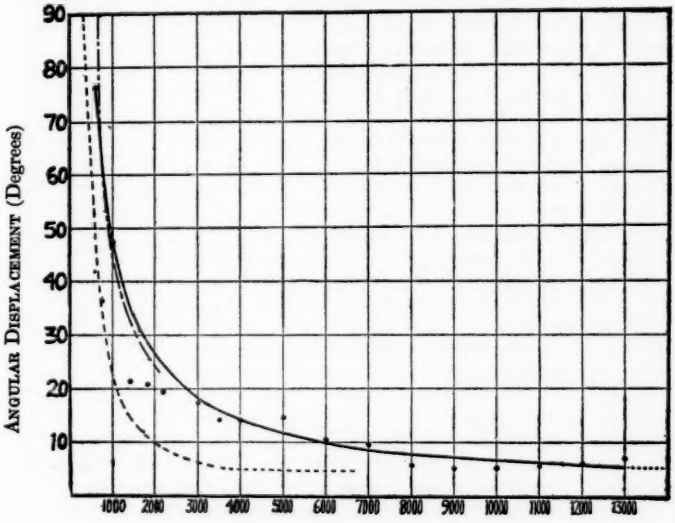

FIG. 1. MAXIMAL ANGULAR DISPLACEMENT OF LOCALIZATION OF DIOTIC TONE, WHEN PHASE RELATION IS ALTERED, AS A FUNCTION OF VIBRATION FREQUENCY OF THE TONE.

The ordinate is the maximal lateral angular displacement from the median plane toward one (or the other) side. The abscissa is the frequency of the displaced diotic tone.

curve should on this ground more nearly approximate our curve.

The dot and dash curve is Hartley's function,[23] and is the result of calculations of the position of the acoustic image for varying differences of phase. We have already noted that observations made on the image while it was in motion gave it a greater lateral displacement, than when it was observed at rest. On this basis our curve should, like Hartley's, begin at 90° on the ordinate instead of at 76.4°.

[21] *Op. cit.* [22] *Op. cit.*, 321-326. [23] *Op. cit.*, 376-385.

Most investigators agree that high tones are localized only with difficulty. In this connection, therefore, it is interesting to review the results and introspections of the observers for the various frequencies with which we experimented. The *O*s experienced no striking difficulties in localizing with the 600, 1000, 1400, 1800, and 2000 d.v. tones. They made fairly consistent judgments for five to eight different positions for those frequencies and their introspective reports indicate the existence of no particular difficulty. The fact that the 1400 d.v. was intense so that the ears 'rang' seems to be the most likely explanation for the slight difficulty experienced for this frequency. At 3000 d.v. the *O*s noted that median localizations were not as easily made as the more lateral ones at 10° and 15°. Four positions were localized at this frequency. From 3500 d.v. up but two positions were localized; that point at either side at which the tone lent itself most clearly for localization purposes. It will be recalled that the *O*s kept the binaurals at the ears for these frequencies and were directed to note the movement of the sound images. The introspections show that the image appeared at one side, became clear and definite, then moved in toward the median plane, where for frequencies between 3500 and 8000 d.v. it almost vanished while crossing the median plane, and then reappeared on the other side, became clear and definite and then gradually diminished in clearness until it disappeared. The diminution in the clearness of the image at the median plane was no doubt due to the decrease in the intensity of the sound at this position, for all *O*s agreed that at this median position the sound was scarcely audible. Localization with the image at rest in the median plane was declared impossible by the *O*s. For frequencies above 8000 d.v. the movements of the image were confined to a small horizontal region at each side of the median plane. Upon one occasion M reported a suggestion of movement across the median plane at 9000 d.v., but there is unanimous agreement by the *O*s that in general the sound was inaudible when the difference of phase of the sound at the ears was 0°. We are then led to believe that it is very difficult to localize in the median plane with tones between 3000 and 8000 d.v., and that median localizations are impossible with tones above 8000 d.v.

All *O*s were able to get right and left localizations for whatever tones were audible through the tubes to both ears. Both P and M got good right and left effects with all frequencies up to and including 13,000 d.v. At 14,000 d.v. the tone for P split up and faded away. It would appear, disappear, reappear, and 'ring' in the ear, but had no definite position. For M the tone was definitely localized, but seemed to spread out over a "number of degrees," and even this 'spread' shifted with a shift in

O's attention. At other times M spoke of the "image as very vague." At 17,000 d.v. neither P nor M could hear the tone through the tubes. H got good right and left localizations for the 10,000 d.v. tone, poorer results with 11,000 d.v., and failed to hear the tone through the conductors at 12,000 d.v. In this connection we must recall that throughout the experimentation weak intensities were used in order to keep the tones as pure as possible.

Double images[24] were observed in the case of all tones by all *O*s. The *O*s observed lateral movements of the image beyond the two-image point[25] when the phase difference was further increased, but inasmuch as these movements also occurred under conditions in which the phase-difference was not altered, it is not certain that fatigue is not a more important factor in producing these more lateral displacements of the tone than is difference of phase itself. Two additional facts seem to support this contention. In work with these very high frequencies, the *O*s were given frequent rest intervals. For instance, after two seconds attention to a tone of 9000 d.v., the image for no apparent physical reason would move either inward or outward slightly and then disappear. It might or might not reappear. Moreover, the tone after reaching the two-image point appeared to expand somewhat, and this expansion gave the impression of greater lateral movement. It appears then that the lateral movement of the tone beyond the two-image point is more illusory than real.

Bowlker reported[26] the presence of as many as three images for some of his higher frequencies, *viz.* 1290, 1675, 2090, 2310 d.v., and four images for 3050 d.v. Our *O*s never have reported more than two distinct images. Occasionally, before the tone was weakened for experimental purposes, the *O*s noted other right and left effects than those usually reported as images. These lateral effects always disappeared when the desired intensity was obtained and were probably due to resonance in the tubes dependent on the intensity of the sound. The *O*s observed that the two images, when present simultaneously, at times gave the impression of drawing together and at other times appeared to be connected by an attenuated strand of sound, with the whole sound mass taking a dumb-bell form. It is quite possible that Bowlker by shifting his attention from the two distinct images to a point between these images 'bunched' this complete sound mass and thus gave it the appearance of a single image. As a matter of fact, Bowlker himself questioned the reality[27] of the third image, thinking it might be due to the "result of attention to the two side images."

[24]Bowlker, *op. cit.*, 323. [25]*Ibid.* [26]*Ibid.*, 325 f. [27]*Ibid.*, 325.

Conclusions

(1) With changes of phase tones of high frequency move over a more limited area than do low tones. This decrease in the amount of movement is rapid at first and then less rapid for the very high frequencies.

(2) Localization, as far as observable right and left effects are concerned, apparently becomes impossible only at the upper limits of hearing.[28] For frequencies below 1400 d.v. the *Os* gave fairly consistent judgments of direction; for frequencies between 1400 and 3000 d.v. localization offers no particular difficulty, if proper allowance is made for fatigue; for frequencies above 3000 d.v. lateral localization effects are observable, but median localization, if not impossible, is extremely difficult to achieve.

(3) Greater lateral displacement accompanies varying differences of phase when tones are observed in motion than when they are observed at rest.

(4) Fatigue in localization of very high frequencies occurs rapidly.

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[28] It has just come to the writer's notice that C. A. Ruckmick (this Journal, 37, 1926, 466) reports "unequivocal results for phase differences" up to 8000 d.v.

APPARATUS FOR THE CONTROL OF STATIONARY LIGHT STIMULI[1]

By DEANE B. JUDD, Cornell University

In an investigation of the Purkinje after-image, it became necessary to construct a special control apparatus. In the experimental work, which will be reported later, the apparatus proved so convenient and reliable a method of studying, not only the rapidly disappearing Purkinje after-image, but after-images in general, that a complete description of it is thought advisable.

The apparatus (see profile diagram, Fig. 1) is an adaptation of the rotary tachistoscope.[2] Light from source (A) is cut off by a rotating shutter or episcotister (F), and gives a periodic illumination to the lantern-slide (J). Recurrence of the illumination at a too rapid rate, *i.e.* at every revolution of the episcotister, is prevented by the action of the electro-magnetically controlled shutter (D), shown in Fig. 2. This constitutes the essential part of the apparatus; the accessory parts have been added to obtain certain other experimental advantages:

(1) A quick release and termination of the light pulse is obtained by placing the episcotister (F) in the focal plane of the condensing lens system (B). In this plane all of the light flux falling on the slide (J) is passing through an image of the filament of the source lamp (A) which is roughly ¼ in. high and wide. The episcotister (F) has only to travel, then, ¼ in. to make the complete change from darkness to maximum intensity of the stimulus and from maximum intensity to darkness.

(2) The range of intensity required in order adequately to study the Purkinje phase is made possible by the insertion of 5 diffusing screens in series (I), the arrangement being similar to that found in the Nagel adaptometer.[3] Continuous variation within the brightness step caused by the insertion of any single diffusing screen is accomplished by means of a neutral absorbing wedge similar to that used in annulus form by Abney[4] in his work on vision.

(3) The control of the surrounding field intensity independent of the stimulus intensity is accomplished by projecting the

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[1]From the Physical Laboratory, Cornell University.

[2]A. Goldscheider and R. F. Müller, *Zsch. f. klin. Med.*, 23, 1893, 134. Cf. also H. L. F. von Helmholtz, *Physiol. Optik.*, 514.

[3]W. Nagel, Zwei Apparate für die augenärtzliche Funktionsprüfung usw., *Zsch. f. Augenhk.*, 17, 1907, 201. Cf. also Helmholtz, *Physiol. Optik.*, English ed., II, 1924, 315-317.

[4]W. Abney, *Researches in colour vision*, 1913, 70.

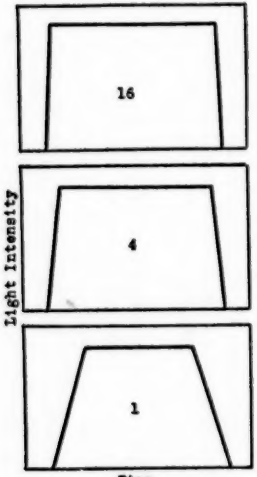

FIGURE 4

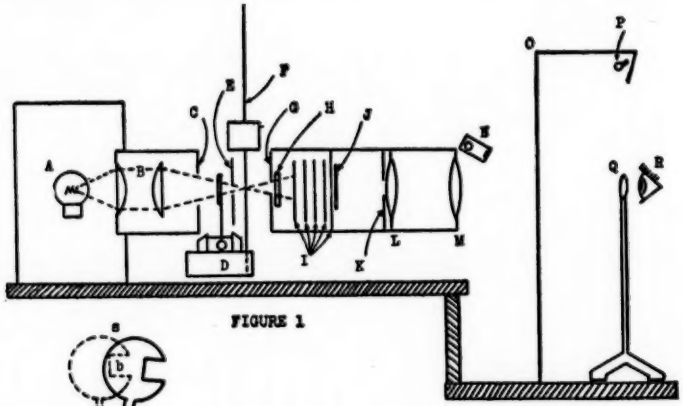

FIGURE 1

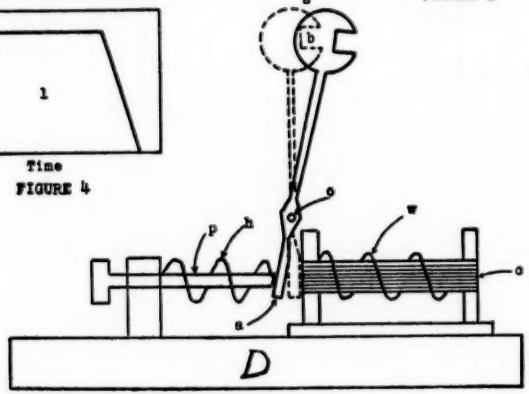

FIGURE 2

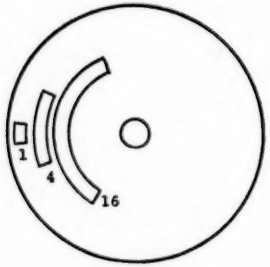

FIGURE 3

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JUMP

image of the slide (J) on the ground glass screen (O) from behind, leaving the surrounding field unilluminated by transmitted light. The surrounding field is, then, illuminated from the front by a series of low-power, incandescent bulbs at (P).

(4) The projection of the slide (J) on the ground glass screen (O) is by means of a system of 2 simple lenses (L and M). Good definition in the projected image is obtained by inserting a diaphragm (K) which cuts off the light from the peripheral portions of the lenses, thus reducing the effects of spherical aberration quite satisfactorily.

Details of Construction

Production of the Illuminating Beam. The source of light (A) was a 100 watt, 115 volt, concentrated filament, gas-filled lamp (Mazda spotlight), run at 110 volts direct current. This choice was made (1) because 110 volts was at all times available, (2) because direct current is more conveniently measurable than alternating current, and (3) because it is more likely that the initial calibration will hold for several months when the lamp is operated well beneath the rated 115 volts. The voltage was measured by a voltmeter and kept constant by means of a slide resistance of 100 ohms in series. The lamp was enclosed in a light-tight, sheet-iron housing, 17 in. high, 12 in. wide, and 13 in. long. The housing was sufficiently large to prevent overheating.

The condensing system (B) consists of the usual two plano-convex, 6 in. lenses, each having a focal length of about 6½ in.[5] The light (A) was placed at the focus of the first lens, the episcotister (F) at the focus of the second, the lenses mounted 4 in. apart.

The diaphragms (C, E, and G), ⅝ in. diam., were inserted to prevent light leaks. The second diaphragm (E) has the additional function of protecting the rather fragile magnetic shutter (D) from accidental entanglement with the rapidly rotating episcotister (F).

Control of Duration of the Illuminating Beam. The electro-magnetically operated shutter (D) is the only unit of the apparatus that was difficult to design. The function of this shutter is to allow the beam to reach the episcotister (F) before its aperture arrives at the light beam and subsequently to cut it off before that aperture comes around on its next turn. The episcotister was used between the limits of 30 and 150 r. p. m., hence the magnetic shutter (D) must be opened and closed within ½ sec. to function adequately. The moving part must therefore be of small inertia. Fig. 1 shows the end of the shutter; Fig. 2, a side view such that the beam to be intercepted lies perpendicular to the page. The movable part of the shutter consists of a light bar of sheet aluminum—an L cross-section to give stiffness—pivoted on a needle suspension at (o), carrying, 5 in. above the suspension, the shade (s), and 1 in. below the suspension, the armature (a). The shade (s) is about 1½ in. in diam. and is made of a double thickness of aluminum foil, which at once gives it durability, opacity, and lightness. The shape of the shade is such that light leakage is reduced to a minimum. The armature (a) of soft iron is ⅛ in. thick by ½ in. square.

[5] If I were to construct anew, I should take a second lens of longer focal length, say 12 in. With the 6 1/2 in. focus, the axial distance within which all the light flux passes through an area of 1/2 in. diam. is only about one inch. Thus the shutters (D and F) and the diaphragms (E and G) all had to come within the space of a single inch. The diagram (Fig. 1) shows these parts apparently more widely separated, a distortion of the true scale which was introduced for the sake of clearness. The introduction of a second lens of 12 in. focus into the condensing system (B) instead of the present 6 1/2 in. focus lens would eliminate this crowding.

The helical steel spring (h) holds the armature away from the magnet against post (p)—as is shown in Fig. 2 in solid lines—when current is not passing through the coil (w). When the circuit is closed, however, the armature (a) is attracted to the magnet core (c)—as is shown in the dotted lines—thus moving shade (s) ¾ in. to one side and allowing the light beam (b) to pass through. The magnet is wound with 400 turns of No. 24 insulated copper wire on a cylindrical laminated core (c) of soft iron ⅜ in. diam. by 2 in. long. With a current of 2 or 3 amperes, the action of the shutter is unfailing and very rapid. Since the coil (w) itself has a resistance of about 2 ohms, which with the external circuit makes a total of approximately 4 ohms, the shutter operates satisfactorily on a 12 volt storage battery.

The episcotister (F) consists of a 22 in. metal disc belted to a direct-current motor of ⅛ h. p. which drives it at regular rates that may be varied at will continuously from 30 to 150 r. p. m., the variation being accomplished by a rheostat in series with the motor. The disc is used for two simultaneous but distinct purposes: (*1*) the timing of the light pulse over a greater range of durations than that of 5 to 1 given by variation in motor speed alone; and (*2*) the control of the magnetic shutter (D) so that the two operate in mutual coördination.

(*1*) The range of durations was attained by fashioning three sector apertures, ¾ in. wide, in the disc (see Fig. 3); the second (4) is of four times the angular extent of the first (1); the third (16), of four times the angular extent of the second (4). The smallest sector was cut out furthest from the center (½ in. from the edge) and is about 1 in. long, or, roughly, of 5 degrees. The second (4) and third (16) apertures are successively closer to the center of the disc. To understand how the light beam is caused to pass through aperture (16) for example, after adjustments have been made for aperture (1), it must be understood that the disc and driving motor are fastened to the table permanently. The other parts of the apparatus, except the ground glass screen (O) and its immediate accessories, are arranged so that they can be moved horizontally in the plane of the rotating disc; or, stated another way, perpendicularly to the light beam. Although, as indicated in Fig. 1, the center of the disc is slightly higher than the center of the light beam, still the more centrally situated apertures can be reached readily by this horizontal motion. To facilitate the adjustment, all these movable parts, except the eye guide (Q), were mounted on a single board which slides on a grooved support in the required direction. For durations as short as 0.005 sec., the smallest aperture is used at 150 r. p. m.; for durations as long as 2 sec., the rotating shutter is removed from the path of the light beam, the light pulse is then controlled by the magnetic shutter alone, and the disc rotated at 30 r. p. m. Fig. 4 shows the rise and fall of light intensity in comparison to the duration of the light pulse for the 3 apertures—1, 4, and 16.

(*2*) The control of the magnetic shutter is maintained through the action of a 5-pointed starwheel which rotates ⅕ turn with each revolution of the disc. The wiring for the magnetic shutter (D) places in series the 12 volt storage battery, a key which can conveniently be depressed by the observer, the magnetic shutter, and a switch operated by the starwheel. The switch controlled by the starwheel is arranged so that it closes at one of the 5 positions of the starwheel but remains open for the other 4. Thus, when the observer's key is depressed, the magnetic shutter opens—and stays open—for one rotation of the episcotister in every five. The lug on the episcotister disc which engages the starwheel is placed 1 in. from the center of rotation; this arrangement is practicable for high speeds as well as low. The starwheel is so adjusted that the time of engagement with the lug corresponds to the end of the light pulse as determined by the episcotister. Thus, the magnetic shutter does not commence to open until

the aperture of the episcotister has been passed and does not commence to close until the same position has been reached on the next turn. In this way a single pulse of the duration expected from the angular aperture and speed of the episcotister is assured.

Control of Intensity of Illuminating Beam. As before stated, continuous variation of the intensity was accomplished by means of a neutral absorbing wedge (H). This neutral wedge, together with a compensating wedge which yields equal transmission over the whole absorbing field, gives transmissions varying from 0.1 to 0.9 depending on what part of the wedge lies in the path of the beam.[6] The wedge is supplied with a scale from which its position may be read.

The combination of diffusing screens (I) used was a milk glass whose transmission in place was approximately 0.01 and four ground glass screens whose transmissions were about 0.2, 0.2, 0.2, 0.4 respectively. The screens were all 5 x 6 in. The inside of the housing containing the diffusing screen was coated with MgO. This served to keep the color of the illuminating beam constant irregardless of the number of diffusing screens used.

To measure the intensity of the image of the lantern slide (J) the wedge and diffusing screens were calibrated in place by determining the intensity of the stimulus image with a photometer pointed through the eye-guide (Q). From this data a calibration curve plotting stimulus intensity against wedge setting was made up for each combination of screens.

Projection of the Slide Image. The two projection lenses (L and M) are 5 in., double-convex lenses of 10 in. focal length. The slide is placed 10 in. from the first lens (L) which is, in turn, 10 in. from the second (M). The ground glass screen (O) is placed to catch a focussed image of the slide which, of course, makes the screen's distance from the second lens (M) again 10 in. To get brightnesses in this image as high as 500 ml. it is necessary to remove all the diffusing screens (I), set the wedge (H) at its highest transmission, and use the entire transmitting surface of the lenses (L and M) without a restricting diaphragm. The resulting image is not perfect on account of spherical aberration. To obtain a uniform field with good definition some intensity must be sacrificed; for, at least, one diffusing screen (I) must be placed in the path of the beam and the diaphragm (K), 1½ in. in diam., must be inserted. The resulting image may be varied in intensity up to about 20 ml. The lower limit of intensity may be extended indefinitely, of course, by the addition of diffusing screens; but if the apparatus is to be used for after-image work alone it seems probable that 0.001 ml.—attainable with the five diffusing screens specified above—is a brightness well below the requirements of any but the most extreme cases.

To make easy the elimination of stray light the absorbing (H) and diffusing (I) systems together with the projecting system (L and M) were enclosed in a light-tight, sheet-iron box. Light enters through diaphragm (G) and leaves through lens (M). The sheet-iron, gauge No. 24, gives a sufficiently rigid construction that the wedge holder (H), the slotted supports for the screens (I), the holder for the slide (J), and for the lenses (L and M) needed no other supporting framework.

Control of Observing Conditions. The screen (O) which forms the field surrounding the stimulus is a piece of window glass, 2 ft. high by 3 ft. wide, ground to an even, diffuse finish on the side toward the observer (R). The grinding was done by hand; a small glass plate was used as a grinding tool with fine emery and water as the abrasive. The source of the surrounding field illumination was a series of four 6 volt, 2 c. p. lamps supplied with

[6] This unit, consisting of a neutral wedge and compensator, is carried in stock by the Eastman Kodak Company, of Rochester, New York.

current from the A. C. power line through a 100 watt, step-down transformer. The lamps were wired so that one, two, three, or all of them could be lighted as desired. A rheostat served to make possible the continuous variation of intensity between these arbitrary steps, without producing objectionable change in the dominant wave-length of the illuminating beams. In this way the background intensity could be varied continuously between 0.5 and 0.0001 ml. The eye guide (Q) is a 1 in. wire circle mounted on a standard 11 in. in front of the screen. At this observing distance the ground glass screen fills most of the visual field. To illuminate the entire surrounding field with approximate uniformity, a screen of gray cloth one foot wide was hung up, extending out perpendicular to the ground glass (O) along its sides, top and bottom.

Behind the screen, but above the beam which projects the stimulus image, is located the unit (N) whose function is to provide a fixation point. This unit consists of a 3 volt, flash-light lamp and converging lens of ½ in. diam. and 1 in. focal length. The lens projects an image of the lamp filament onto any desired part of the ground-glass screen (O). So that no disturbing effects will ensue a red bulb was chosen, and the 3 volt lamp operated at 2 volts or less. The intensity was controlled by a rheostat wired in series with the lamp.[7] When the red image of the lamp filament is reduced in intensity so as to be just definitely above the threshold, it is entirely blotted out by the stimulus and the succeeding after-images, and does not interfere with their observation.

Summary. The principle advantages of the apparatus may be summarized as follows:

(1) A light pulse is produced whose duration is continuously variable between 0.005 and 2.0 sec. The rise from zero to maximum intensity and the subsequent cut-off of this pulse is so rapid as to be negligible in comparison to the duration of the pulse.

(2) The intensity of the light stimulus is continuously variable from 0.001 up to 500 ml.

(3) The surrounding field intensity is continuously variable from 0.0001 up to 0.5 ml.

(4) A sharp image may be projected of any object that may be put on a lantern slide.

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[7]These are the devices used for intensity control of fixation point by Fröhlich in his study of after images, *Zsch. f. Physiol.*, 53, 1921, 81-82.

SOME NEW LABORATORY AND DEMONSTRATIONAL APPARATUS

By Edwin B. Twitmyer and Samuel W. Fernberger,
University of Pennsylvania

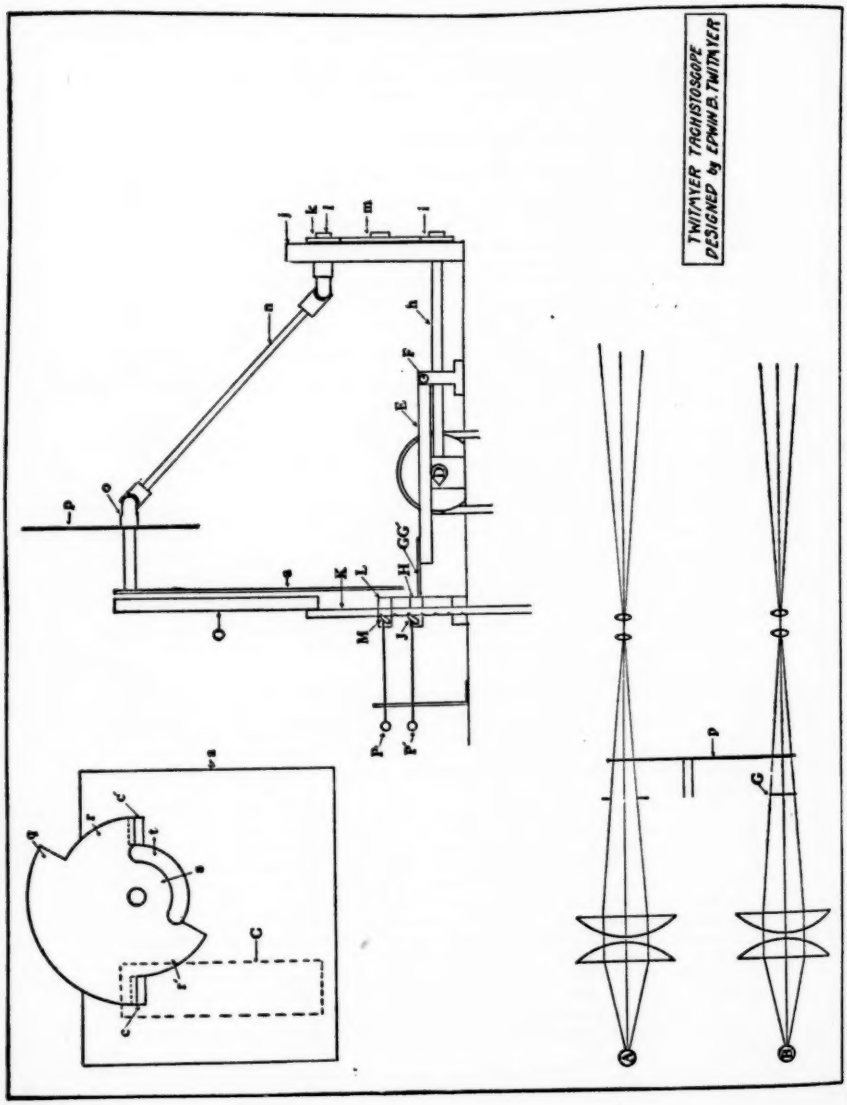
The Twitmyer Demonstration Tachistoscope

The problem of serial exposures for demonstration purposes before a large group has not heretofore been successfully solved although a number of devices have been developed for this purpose. Several factors are essential for such an apparatus. (1) It must produce an image large enough and clear enough to be seen from a distance and so arranged that the subjects sitting far away will have an unobstructed view of the material. (2) The apparatus must provide for regularity of exposure. (3) It must provide for a pre-exposure and a post-exposure field. (4) It must provide for changes in the length of the exposure. (5) It must provide for a sufficient number of regularly timed successive exposures for memory work. (6) It must not disclose the changing of the material.

The apparatus, to be described, provides a clear and intense image which can be seen clearly for a distance of more than 100 feet. It provides for a pre-exposure and a post-exposure field, as well as for 12 equally timed exposures. The times of the exposures can be varied. While the material is being changed between exposures, the subject sees a field similar to the pre-exposure field.

This apparatus is built around two projection lanterns of the usual type. One lantern (A) provides illumination on a screen between exposures (while the material is being changed) and the other lantern (B) provides illumination of the materials. These two fields are focused so that they exactly coincide on the screen and the effect is of a single illuminated exposure field in which materials appear and disappear without spatial movement.

This result is accomplished by a motor and speed reducer which revolves a shaft, at the end of which is a sharp pitched cam (D). When this cam revolves, it pushes up an arm (E) which is pivoted at one end (F) and carries two fingers at the free end (G and G_1). These fingers press up a slotted carrier (H) which moves on two guides set on a central upright. (H) carries a pawl (J) which engages in a slot in the rack (K) and moves this rack up to the extent of the excursion of (H). A second carrier (L) is placed permanently above (H) and carries a second pawl (M) which engages in the slot of the rack (K) just above that engaged by the pawl of the carrier (H). This prevents the rack from falling back to its former position when the carrier (H) returns on its down stroke. This return of the carrier (H) is accomplished by two springs placed between the carriers thus providing a more positive action than could be accomplished by gravity alone.


TWITMYER TACHISTOSCOPE
DESIGNED by EDWIN B. TWITMYER
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Above the rack (K) and fastened to it is a frame (O) which is the holder of the photographic slide (C) holding the material to be shown. By means of the mechanism just described, the materials on the slide are brought successively opposite the focus of the projection lantern (B). In order to return the slide to its first (pre-exposure) position, it is only necessary to disengage the two pawls from the rack and gently lower the rack, frame and slide by hand. The pawls are engaged in the teeth of the rack by two springs. They are disengaged by two levers (P and P′) working against the springs.

A permanent screen (a) is fastened to two uprights. In this screen are cut two rectangular holes (c and c′) before the focus of each lantern. These control the size and shape of the exposure field acting as a diaphram. The horizontal relation between the screen (a) and the frame (O) is permanent. The vertical relations between these two may be altered by moving a screw, which changes the relations between a permanently attached standard and a standard at the top of the screen (a).

The exposure shutter is actuated from the same driving shaft, which actuates the cam (D), which in turn raises the rack (K). Thus the synchronization of the movements of the shutter and the movements of the rack and the frame (O) is accomplished. Fastened to the middle of shaft is a miter gear which engages in another miter gear. The second gear is permanently attached to a shaft (h) which is thus made to revolve. At the other end of shaft (h) is a spur gear (i). An upright (j) carries another spur gear (k), attached to the outer end of another shaft (l). In order to raise gear (k) so it will be more nearly opposite to the center of the shutter, another large spur gear (m) is inserted between (g) and (k) and transmits the power from one to the other. At the end of shaft (l) is a universal joint which is also fastened to another connecting shaft (n). Another universal joint connects this connecting shaft (n) with the shaft (o) which is inserted in the screen (a). On shaft (o) is permanently attached the shutter (p), and the mechanism just described causes it to revolve synchronously with the movements of the rack (K).

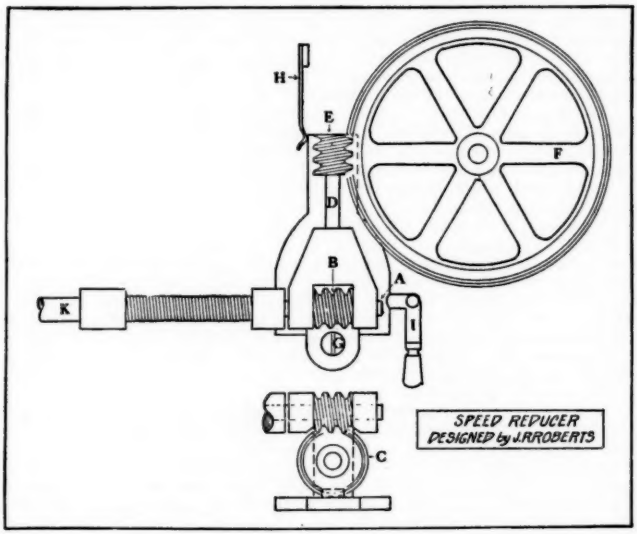
The shutter (p) is cut from sheet metal and is of the shape indicated in the drawing. There are three lengths of radius—(q) is the longest; (s) is the shortest; and (r) and (r′) are intermediate. The circumference for the distance of radius (q) is ⅓ of the total circumference; that for (r) and (r′) combined is another third and that for (s) is the last third. The holes in the screen are so arranged that the beam of light from lantern (A)—the inter-exposure field—is cut off by radii (q), (r) and (r′) and is only exposed while radius (s) is passing in front. The beam of light for lantern (B)—the exposure field—is cut off only by radius (q) and is exposed while (r), (r′) and (s) are passing in front. Hence, when the exposure is being shut off from lantern (B), the inter-exposure field comes on, and vice versa. The ratio of the length of exposure to the time between exposures is obviously 2:1. In our present arrangement, the total revolution of the shutter gives an exposure at the rate of 1 per sec. This means that the material is actually exposed for ⅔ of a second and that the inter-exposure time is ⅓ of a second, without a break in the light of the total exposure field. A counterweight (t) aids in the balance of the shutter.

It has been necessary to have an accessory apparatus for the taking of the slides in order to insure that each stimulus will come exactly in the same place in the exposure field. This device involves the use of an enlarging camera with a slide held in a frame, similar to the frame (O). The slide is mounted on a rack exactly similar to rack (K), which is held in position by a hand operated pawl. The first item to be photographed is projected into the enlarging camera with the slide as far down as possible (leaving room for the pre-exposure field) and is thus photographed. The rack is then pushed up to the second notch and the second material photographed; and so on.

It is possible to use the rack and pawl device for individual exposure in research without the use of the lantern by placing the subject in front of the machine and faced toward it. In such a situation it is possible to dispense with the use of photographic plates and to substitute materials on cardboard.

The Roberts Speed Reducer

The kymographs used in the laboratory work at the University of Pennsylvania are of the motor driven type. Until



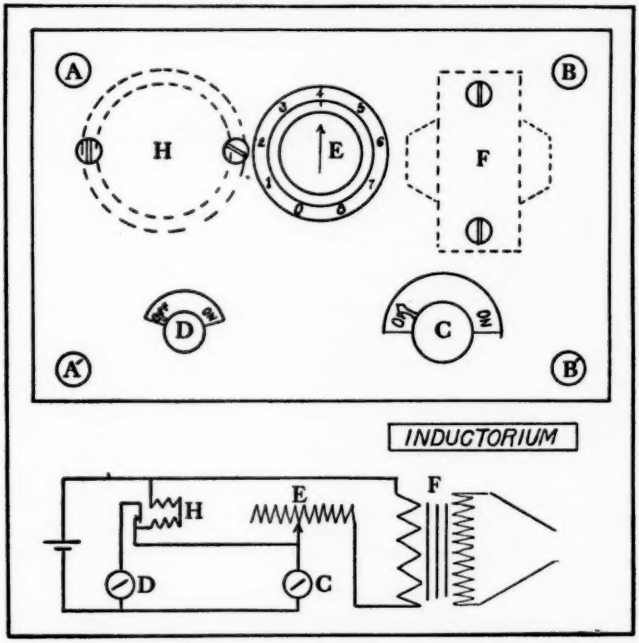
Roberts Speed Reducer

recently the motor speed has been reduced by a speed reducer of the belt-wheel type involving a series of eight pairs of wheels. This has been found to be too large, heavy and cumbersome for practical use. It also involves continual adjustment of the belts to avoid slipping. Hence the speed reducer now described was devised to obviate these difficulties.

The overall size is 4.5 x 2.25 x 2.25 in. The speed reducer is mounted directly on the base of the old kymographs. It consists of a cast brass base, holding two shafts set at right angles to each other. One end of shaft (A) is attached to the motor (at K) by a shaft drive or a flexible spring drive—which is shown in the drawing—and at the other end is a worm (B). The worm (B) engages in a gear of 20 teeth (C) which is attached to the end of the second shaft (D) which, in turn, is set at right angles and below shaft (A). At the other end of shaft (D) is another worm (E), similar to

the one (B) already described. The worm (E) engages in the teeth of a large gear (F) with 80 teeth, which is rigidly attached to the upright driving shaft of the kymograph.

The pulley is removed from the motor and the flexible driving shaft is placed directly over the shaft of the motor (K). Thus the speed is reduced from 1,750 revolutions per min. of the motor shaft to about one revolution per min. of the kymograph drum. The speed reducer is fastened to the base of the kymograph by a single screw (G). Worm (E) is held in contact with gear (F) by a spring (H). The entire speed reducer is actuated against



ROBERTS INDUCTORIUM

spring (H) by a handle (I), so that worm (E) may be disengaged from gear (F), thus stopping the kymograph drum with the motor and speed reducer running. The kymograph drum is again started by pulling the handle (I) and allowing the spring (H) to re-engage worm (E) and gear (F). Grease cups are provided for the bearings of shafts (A) and (D).

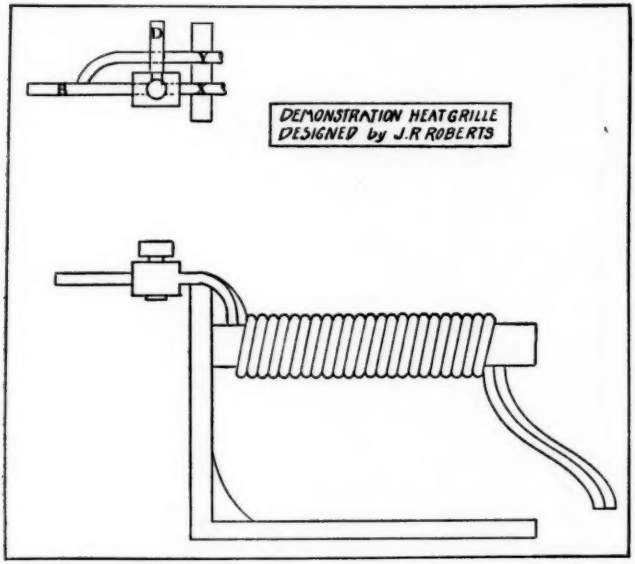
The worms and gears used in this apparatus are stock parts sold by the Philadelphia Gear Works, Philadelphia, Pa. The gears are Catalogue numbers 835 and 839. Steel worms were used to fit these gears. The price of all gears and worms was $4.50.

This speed reducer recommends itself not only for the advantage in size and weight, but also in that the entire mechan-

ism is a direct drive without the possibility of slowing or stopping as in the older friction driven or belt driven machines.

The Roberts Inductorium

A useful laboratory inductorium is contained in a box 8 x 5.5 x 5.5 in. It is made entirely of stock electrical parts. Mounted on a Bakilite panel are two sets of binding posts



Demonstration Heat grille

(A and A′) leading in from the source of current—a dry cell—and (B and B′) leading out to the electrodes. Two switches are also mounted on the top of the panel—one (C), for simple make-and-break (a Marco snap switch) and one (D), for faradic current (a Yaxley switch). A rheostat knob (E) controls the intensity of the current.

The parts consist of a transformer (F), such as is used in a household for ringing a bell, with a ratio of about 1:12; a 25-ohm rheostat (G) such as is used in radio for controlling the filiment of a vacuum tube; and an ordinary commercial buzzer of the nutmeg type (H). The purpose of the transformer is to set up the current or to increase the voltage. The rheostate controls the intensity of the current by introducing more or less resistance. The buzzer is the means for rapidly making and breaking the current. The wiring is indicated in the accompanying diagram.

To obtain simple make-and-break stimulation, one merely throws switch (C). To obtain a faradic current, one merely throws switch (D). The total cost of such an inductorium should be less than $6 at the present prices and the saving in space and cumbersomeness is considerable. This form of inductorium obviates the necessity of adjusting the vibrating hammer of the older type of apparatus. Also the parts are permanently enclosed in a box which materially lessens the chances of injury due to handling.

A Demonstration Heat Grille

A convenient form of demonstration heat grille is made by coiling copper tubing of ⅛ in. bore with walls 1/16 in. thick about a horizontal circular wooden rod of ¾ in. diameter. Two tubes are used and these are coiled alternately for approximately 6 in along the wooden bar. The advantage of this form of heat grille is that there are no sharp shoulders in the tubing with the consequent constriction in the tubes which interferes with the flow of water.

Arrangements are made to connect tube (D) with a tank of cold water and tube (B) with a tank of warm water. The following arrangement of a two-way cock is easily constructed. Warm water flows continually through tube (Y) from the inlet (B). When the cock is in the position (BX) warm water also flows through tube (X). By turning the cock to the position (DX) the warm water is shut off from tube (X) and the cold water is allowed to flow through tube (X) without affecting the flow of warm water through tube (Y). This has the advantage over the older form of heat grille inasmuch as some water flows through both tubes continually. The present form of heat grille is adapted to stimulating the palm of a hand which simply grasps the tubes (X) and (Y). Although there are not as many temperature spots on the palm of the hand as elsewhere on the surface of the body, this apparatus, with the dimensions indicated has never failed in arousing the synthetic experience of 'hot.'

AN IMPROVED APPARATUS FOR ACTUATING TONAL CYLINDERS OF THE KOENIG TYPE

By SAMUEL W. FERNBERGER, University of Pennsylvania

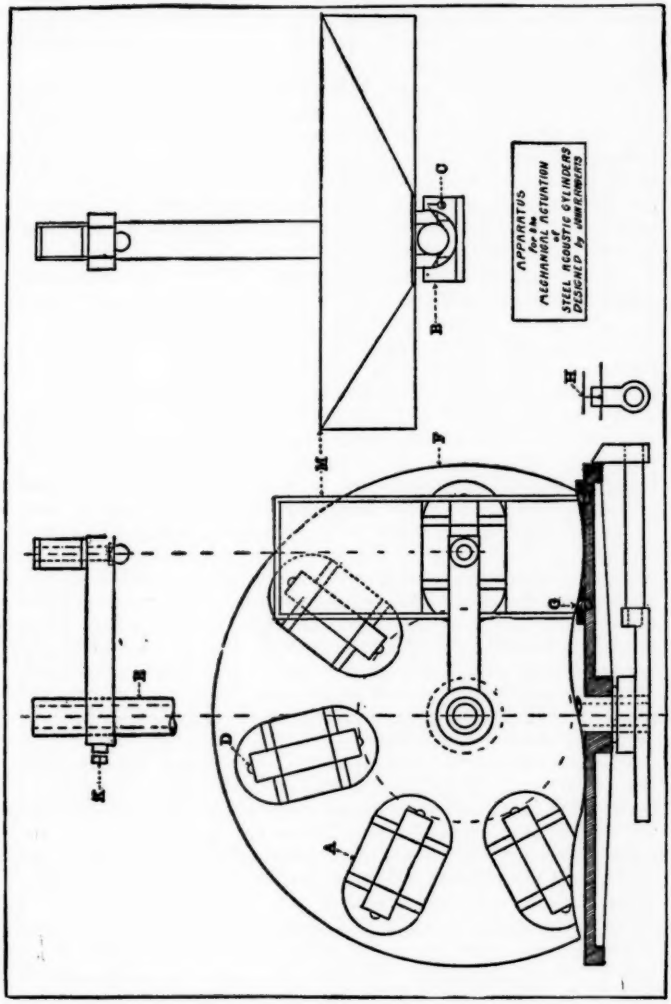
The use of Koenig cylinders for the determination of the upper limen for tone is widespread. Their use in research problems has not been eliminated by the introduction of the radio frequency oscillators more recently devised, and their use in clinical procedure is indicated. The actuation of these cylinders has always been a problem for research. In clinical procedure, the usual method has been to suspend the series of cylinders in a metal frame by dental floss or some other strong but fine thread and to strike the cylinders with a small metal hammer. It is obvious that one cannot control the intensity of impact by such means although the procedure may be adequate for clinical practice.

The earlier method for research was to suspend the cylinders used in a frame similar to the one described for clinical work but to actuate them by the blow of a metal ball released by an electro-magnet. The intensity of the blow was controlled either by the weight of the ball or the height of the fall or by a combination of both of these factors. The difficulty of such a procedure was to bring the cylinder under the ball.

This difficulty was partially eliminated by Möller[1] who mounted the cylinders in a semicircular trough. The electromagnetic release for the actuating ball was attached to a vertical rod mounted in the geometrical center of the circular trough. The electromagnet could thus be swung into any radial position and so could be brought directly over any one of the cylinders. Such an arrangement eliminated many of the difficulties connected with experimentation with the cylinders of the Koenig type. There remains the difficulty, however, of possible spatial localization and hence of judgments on the basis of this secondary criterion. In some early experimentation, we believed that this factor of spatial localization might have a greater influence in a long series of judgments than might have been believed *a priori*. This point was not investigated experimentally but we believed it best to eliminate any possibility of such an influence.

Hence the following apparatus was devised by the author and Mr. John R. Roberts at the University of Pennsylvania. It consists of an adaptation of the turning-top table which has

[1]E. F. Möller, The effect of change of intensity upon the upper limen of hearing, this JOURNAL, 33, 1922, 570-577.


APPARATUS
for the
PNEUMATIC ACTUATION
of
STEEL REGISTER CYLINDERS
DESIGNED by JOHNSON&CO
C
B
H
M
F
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been used for many years for the presentation of lifted weight stimuli[2] to our present problem. The set of cylinders, for which the apparatus was devised, consists of a series of 40 cylinders ranging from 16,000 d.v. to 24,000 d.v. with intervals of 200 d.v. Our first problem had to do with the handling of the individual cylinders inasmuch as different subjects have different liminal values which obviously necessitates the employment of different cylinders from our total series. In dealing with apparatus such as this, it is advisable to handle the cylinders as infrequently as possible to avoid rusting. Hence we mounted each cylinder in a saddle and it could then be placed in the stimulating apparatus without handling the cylinder itself.

These saddles, indicated in top and end view on the accompanying drawing as (A) and (B) respectively, show the cylinders in position. They were made of aluminum castings, each of a floor plate with two uprights toward either end. These uprights are cast with an almost semicircular hole cutting away most of the metal of the uprights to accommodate the cylinder. Three holes are drilled in the sides of each upright. One of these is tapered to accommodate a taper pin (C) and two small holes are drilled to take each end of the dental floss used to suspend the cylinder. The dental floss is inserted in these holes and knotted at one end. The other end is passed through a hole in the taper pin and fastened.[3] Once the two strings have been adjusted in the saddles in the manner indicated, the grooves in the cylinders were placed over them and the height of the cylinder from the floor of the saddle was adjusted by screwing the taper pin in the same way that one adjusts a violin key. Two holes (D) are drilled in the floor plate of the saddles which may engage in two short pins permanently fixed in the apparatus. Thus the saddle one wishes to employ is readily removed or placed in exactly the proper position by merely lifting it up or setting it down. Inasmuch as the different tones of the cylinders are entirely a function of their length and, inasmuch as the position of the grooves of the cylinders are a function of their total length, it was found necessary to develop saddles with different degrees of separation of the uprights. All of the base plates are of exactly the same dimensions. With our present set of cylinders, which is a rather extended series, it was

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[2]Cf. F. M. Urban, *The application of statistical methods to the problems of psychophysics*, 1908, 3-4; S. W. Fernberger, On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 14, 1913, (Whole No. 61), 10-11.

[3]For the use of taper pins in making fine adjustments of the length of cords as in the case of the Sanford chronoscope, cf. S. W. Fernberger, Improvements on the Sanford vernier chronoscope, this JOURNAL, 37, 1926, 154-155.

found that only three saddles of different dimensions were necessary. The inside separation of the uprights in the present set measure 3.6, 3.8 and 4.2 cm.

The main part of the apparatus consists of a base which rests flatly on the table. Vertically through the center of this is inserted a steel cylindrical tube (E) 2.5 cm. in diameter with a 1.3 cm. hole. Rotating about this tube is the turning table proper (F) which is made from a single aluminum casting, 37 cm. in diam. Seven pairs of short pins are inserted in this casting (G) along equidistant radial lines over which the saddles with the cylinders are placed, as described above. Lines are cut on the edge of the table (F) just on the line of the radial pins and these lines are brought opposite to the sighting line (H) when the table is used. In this fashion, each cylinder may be brought into proper position in relation to the electromagnetic actuator. This sighting line is placed 30′ to the right of the radial lines.

The electromagnetic release is held on a horizontal arm which is attached to the upright (E) by a set screw (K). Thus. it may readily be raised or lowered by a very simple adjustment. In order to insure the correct lateral position of the electromagnet, the point of the set screw is placed in a longitudional slot cut into the upright (E) which is just exactly opposite to the sighting line (H). By this adjustment, one is assured that the ball will hit the proper place on the cylinder, provided the lines on the table opposite each saddle are brought exactly opposite the sighting line (H). A small piece of soft rubber ring is inserted below the electromagnet so that the metal ball is brought into the field without actually touching the magnet proper. The wires from the magnet are run down the hole in the tubular upright (E) and thence to the floor so that the circuit may be broken by a key actuated with the foot. The arrangement leaves both hands free for handling the swinging of the table, for placing balls in the electromagnet and for recording.

If the cylinder is in proper position with regard to the electromagnet, the ball will fall on the cylinder exactly in the middle (from end to end) but a little off center from the top. It will therefore bounce to one side. In the present apparatus, a felt covered metal frame has been mounted on the upright (E) in such a position just above the cylinders that the ball will bounce directly into it entirely noiselessly. This frame is indicated in the drawing by the letter (M).

The apparatus just described is very easily manipulated and the lines on the edge of the turning table are brought to the sighting line with a surprising degree of accuracy and with a small amount of training. The cylinder which is actuated is thus always in the same spatial relation to the subject for all

successive stimulations. We have found, for our present purpose, that seven cylinders correctly chosen are sufficient for the determination of either a stimulus limen or of a difference limen. If more cylinders were desired, it would be a very simple matter to enlarge the entire plan of the apparatus to accommodate the additional cylinders.

This apparatus, with the addition of another electromagnetic release device, set along another radial line, and of another sighting line below it would readily lend itself for the attacking of such problems as the effect of intensity on the upper limen for tone. Balls of the same weight would be used and the differences of intensity of the impact would be secured by setting the electromagnets at different heights on the upright (E). Hence the two series of different intensities could be mixed, and the judgments made under exactly similar conditions—a problem which has been difficult of solution heretofore.

THE CONSTRUCTION AND CALIBRATION OF KOENIG CYLINDERS

By Edwin G. Boring, Harvard University

Mr. A. G. Cox of the Harvard Psychological Laboratory has recently constructed two sets of steel cylinders of the Koenig type. One set is like the set described by Fernberger in the preceding article of this Journal.[1] It consists of 41 cylinders ranging from 16,000 d.v. to 24,000 d.v. by increments of 200 d.v., and is used for the accurate establishment of the psychometric function that determines the upper limit of hearing. The other set consists of 22 cylinders for the notes of the three upper natural diatonic octaves, ranging from $g^4 = 3072$ d.v. to $g^7 = 24576$ d.v. ($c^1 = 256$). This set is useful for demonstration since it extends the musical scale from a region, which is obviously musical and where the tones are loud and well sustained, up to and beyond the limit of hearing. We have thought that with this instrument we might come at a way of discovering whether, where, or just how the tones of the upper audible region lose the musical properties that are involved in fusion, consonance, and the perception of musical interval, although we are not yet ready to assert that the present instrument is adequate to this problem.

At present both of these sets have the cylinders suspended at the nodal points by loops of thread from pins on two diverging bars fixed to a base. It is obvious that for accurate psychophysical work we shall need to construct Fernberger's apparatus.

The main purpose of the present note is to describe the construction and calibration of these cylinders. The frequencies must be calculated and can not readily be measured directly.[2] The formula is: $f l^2 = k$, where f is the frequency, l the length, and k a constant that depends upon the material, the units of f and l, and other factors. The usual practice is to assume that k is constant for a given steel bar of constant diameter, to calibrate a long standard length of this steel, and then to compute the desired cylinders from the standard calibrated bar. If F and L be respectively the frequency and length of the calibrated standard and f and l the frequency and length of any cylinder that it is desired to construct, we have, since both are equal to k, $f l^2 = F L^2$; or, for a given f, the length should be, $l = L\sqrt{F/f}$.

Two objections have been offered to this procedure. One is that it is not safe to regard the steel as perfectly homogeneous, that the constant for one section of a rod may not be the same as the constant for another section. The other objection is that theory should not supplant observation and that calculated frequencies can not be trusted in default of direct observation of the actual cylinders.

As bearing upon the first of these objections, we find that difference of kind of steel makes a slight, but not a great, difference. We cut three bars, X, Y, and Z, each 630 mm. in length, from 13/16″ round steel. For these bars we determined the frequencies, suspending them by loops of twine at the nodal points (¼ the total length from each end), sounding them by striking with a rubber hammer, and then counting beats against a Stern

[1] S. W. Fernberger, this Journal, 38, 1927, 120-124. Fernberger's set was made to order by the Standard Scientific Co. of New York (cf. C. C. Pratt, this Journal, 30, 1920, 403-406), but this firm has discontinued the manufacture of acoustic apparatus. As far as I know, these cylinders are no longer being made by any firm in America.

[2] The high tones are not sustained; the calibration by beats or by difference tones is therefore difficult and can not of course apply above the limen of the observer. Schwendt's claim that the Kundt dust method is satisfactory does not seem to be substantiated; cf. Pratt, *loc. cit.*

variator, which was in turn calibrated by beats with a Koenig fork. The error in measurement of the lengths of these rods is not greater than 0.3 mm; we did not seek to be more exact. The frequencies are accurate to within 0.3 d.v., since this is the extreme range of variation for different counts of beats at the rate of about 3 beats per sec. X was hot rolled steel of high carbon content. Y was cold rolled steel with further specifications unknown. Z was cold rolled steel known to have a low carbon content as compared with X. The comparison is as follows:

	X	Y	Z
l:mm.	630.	630.	630.
f:d.v.	242.4	239.7	239.7
k: constant ($= fl^2$)	95,136,930	96,208,560	96,208,560

These results were checked by the observations that Y and Z did not noticeably beat with each other, and that X and Y gave on the average 2.9 beats per sec. with each other, whereas their calibrations with the variator would require 2.7 beats per sec. It is not, however, as satisfactory to beat two rods against each other as to beat a rod against the variator.

Thus it appears that two random samples of cold rolled steel, Y and Z, happened to give identical results within the accuracy of these observations, and that two contrasting samples of steel, X and Z, gave results differing by only a little over one percent, a difference probably depending on the character of the steel, since it is several times the error of observation. It would seem, however, that we need not expect a large change of the constant due to heterogeneity of the steel in rods cut from a single bar of high-grade stock advertised by its makers as "homogeneous." Errors in frequencies thus computed should not, on this account, be more than a tenth of one percent and may be much smaller.

The foregoing data also bear on the error in frequency introduced by inaccuracy of machining the length of a bar. With X, 1 mm. = ca. 0.75 d.v. Since there is no difficulty in machining a long rod more accurately than to even millimeters, it is plain that accuracy is easily established to less than one d.v. At about 16,000 d.v., with steel like the steel of Z, 1 mm. = ca. 400 d.v., but 0.1 mm. = ca. 4 d.v. Since it is possible with a micrometer to machine the short cylinders to less than 0.1 mm., the error on this account may be as little as 1/40 of one percent. We cannot, of course, assume that it is actually so small since other sources of error may enter in.

The validity of the formula can be checked in the middle tonal range where computation can be compared with observation. For this purpose we took the rod Z, described above, and cut it down from 630 mm. to 431 mm., a length that by calculation should give 512.8 d.v. By beating this bar against a Stern variator, which was calibrated in turn by a Koenig fork, we determined the frequency of the shortened rod as 512.6 d.v. The difference of 0.2 d.v. is within the error of observation by beats, and we assume the verification of the formula.

It is customary to turn grooves in these cylinders at the points of suspension in order to hold the suspending loops. It is not clear, however, what effect these grooves have upon the calculated frequencies. That the removal of metal may have an effect first appeared when we turned two grooves in rod Y, not at the nodal points, but (as the result of an error) quite near each end. The removal of this metal changed the frequency of the rod from 239.7 to 243.0 d.v. Our most accurate test of the effect of grooving at the nodal points was made with rod X. This rod was first cut to 608 mm. and gave then 257.9 d.v. (thus checking the formula). We then turned two V-shaped, 60° grooves at the nodal points. The grooves were about 1 mm. wide at the top and 1 mm. deep. Recalibration of the rod then gave 256.9 d.v.; the pitch had been lowered about 1 d.v., an amount significant as against the error of observation.

It is not possible to say what the effect of the grooves in the short cylinders would be, but it is plain that the same size of groove (a size usual in the construction of these cylinders) would remove relatively more metal. For this reason we have, in our two sets, made the grooves very light. We have done little more than score the surface. The grooves were made with a standard 60° thread tool, which was let into the surface about 0.01 in. Such a groove keeps the thread suspension in place; the cylinders can swing violently in the longitudinal direction without the loops slipping.

Our actual sets of cylinders were constructed from the cold rolled steel used for rod Z.[3] We used the shortened Z for a standard, *i.e.* the length of the standard was 431.0 mm. We are convinced that this measurement is correct to 0.1 mm. for we measured it, using end-stops and a lens, on a standard steel scale graduated to half-millimeters. We calibrated it by beats against a Koenig fork (256 d.v.), a Western Electric audiometer, type 2A, set for 256 d.v., and the Stern variator, whose adjustment had been altered since the previous calibration. The results, accurate to within a third of a beat per sec., were as follows:

Standard of reference:	Koenig fork	Audiometer	Variator
Indicated frequency of standard rod: d.v.	506.6	510.3	515.0

The differences in calibration by the three standards were checked by observing that the standards beat with each other as indicated. The result from the variator can be discarded since the instrument is adjustable and is not an absolute standard. The audiometer beat with the Koenig fork and it is impossible for us to say which, if either, was correct. We computed our sets by reference to the Koenig fork. If the audiometer is correct, our calibrated frequencies should be multiplied by 1.0073. With the diatonic series this uncertainty is not important, since only the relative frequencies are of primary interest. With the fine series for the determination of limens the error introduced may be more than 100 d.v., *i.e.* more than half the increment between adjacent cylinders. It is less, however, than the probable error of the limen,[4] and is of course relatively as accurate as accepted standards for the middle range, like the Koenig forks.

The constant of this steel stock, based on the Koenig fork, is $k = fl^2 = 94{,}106{,}523$. This gives for the extremes of each series the following lengths:

Diatonic series	Liminal series
g^4, 3072 d.v., 175.0 mm.	16000 d.v., 76.7 mm.
g^7, 24576 d.v., 61.9 mm.	24000 d.v., 62.6 mm.

All the cylinders, except the longest of the diatonic series, were measured with a micrometer caliper and are accurate to less than 0.1 mm.

In the liminal series the function of the lengths deviates so little from linearity, that two diverging straight bars provide adequate suspension. In the diatonic series it was found better to bend each bar slightly in the middle so that the curve of lengths is approximated on each side by two straight lines.

In the use of the cylinders we have found no reason to believe that they are inferior to sets manufactured by professional instrument makers. It is apparent then that anyone can readily have a set made for himself and to his own specifications. All that is necessary is for him to secure lengths of a good cold rolled steel, calibrate a long bar by any standard acceptable to him, measuring its length accurately, compute the desired cylinders from these data, and then have a good machinist cut the cylinders on a lathe, using a micrometer to assure accuracy of length.

[3] The steel used was cold rolled, open hearth, 13/16", round, steel shafting, designated as SAE 1020 by Edgar T. Ward Sons Co. of Boston, who supplied it.

[4] Cf. E. F. Möller, this JOURNAL, 33, 1922, 573 f. For example, if the data for all intensities be combined and Urban's procedure be applied, we get for observer M, her most precise *O*, the upper limen = 18,319 d.v.; h = 0.003076; and hence P.E. = 155.d.v. With observer B, if the inversion, although significant, be left in, we get the limen = 18,844 d.v.; h = 0.000956; P.E. = 499.d.v.

PERSONALITY AS REVEALED BY THE GALVANOMETER

By EDWIN G. FLEMING, Columbia University

In attempting to find an objective measure of personality the galvanometer was suggested as a possible means of obtaining an index. The skin resistance was obtained from 18 *Ss*, all of whom were graduate students of at least one year standing in the department of psychology at Columbia University. These values were then correlated, because of the want of a better criterion, with the subjective ratings of four 'judges,' all of whom knew the *Ss* intimately. Three of the judges (L, G, and W)—members of the faculty who had come, in the class room, the laboratory, and in private conferences, into close contact with the *Ss*—ranked them upon the basis of their 'magnetic personality,' and 'nervous temperament.' The fourth judge (S)—a fellow student of the *Ss*—ranked them only upon the basis of their 'nervous temperaments.'

The instructions were given to the judges in writing and were as follows: "Please rank the following students on the basis of 'magnetic personality,' placing the most 'magnetic' at the top; and also rank them on the basis of 'nervous temperament,' placing the most nervous at the top." Then followed the names of the 18 *Ss* in random order. None of the judges, at the time of making the ratings, knew the values of the galvanometric readings which had previously been secured.

The initial resistance, measured in ohms, of the skin to the passage of a current from a single dry cell was secured for every *S*. All readings were taken at noon just before lunch. The electrodes were physiological salt solutions into which the *S* placed the index and the second finger of the right hand. The *Ss* were ranked upon the basis of these readings—the first position being given to the *S* whose resistance was the highest. Correlations were then computed by the rank method between the resistance ranking and the rankings given by every judge individually and by all combined. The method was to add the rankings of the several judges, and to establish from these totals a new rank order. Correlations were also calculated between the rating of the same judge and of the different judges on 'magnetic personality,' and on 'nervous temperament.' All the correlations were converted into the Pearson r equivalents.

The following tables show the results in terms of Pearson r equivalent of *rho*.

TABLE I

Correlation between Electrical Resistance and 'Magnetic Personality'

	Judges				
	L	G	W	S	ALL
r	.67	.22	.28	—	.44

TABLE II

Correlation between Electrical Resistance and 'Nervous Temperament'

	Judges					
	L	G	W	S	ALL	L, G, and W
r	.53	.31	.14	.32	.40	.35

TABLE III

Inter-Correlations between the Rankings of the Judges on 'Magnetic Personality'

Judge	L	G	W
L		.66	.65
G	.66		.66
W	.65	.66	

TABLE IV

Inter-correlations between Rankings of the Judges on 'Nervous Temperament'

Judge	L	G	W	S
L		.66	.65	.27
G	.66		.67	.73
W	.65	.67		.47
S	.27	.73	.47	

TABLE V

Correlations between Ratings of 'Magnetic Personality' and 'Nervous Temperament'

	Judges			
	L	G	W	ALL
r	.47	−.36	−.34	−.18

A comparison of the rankings of the judges with one another and with the ranking in accordance with initial resistance shows, with the exception of the rankings of a few *Ss*, a close agreement. The average difference in the final combined ranking of the three judges on 'magnetic personality' and the ranking according to initial resistance is 4.3; one of the differences is 13 and another 9; while eight of the rankings show a difference of not more than 2. It was, therefore, thought possible that a higher correlation might be obtained with the judges' rating of 'magnetic personality' if the *Ss* were ranked in accordance with the deviation of their initial resistance from the average. But this method yielded a Pearson r equivalent of *rho* by the rank method of only .30; whereas the correlation when the *Ss* were ranked with the highest resistance at the top and the lowest resistance at the bottom was .44.

By the method of partial correlation, when the combined rating of 'nervous temperament' of the three judges, L, G and W, is held constant, the correlation between the initial resistance and the combined estimate of 'magnetic personality' is increased to .56. When 'magnetic personality' is held constant, the correlation with 'nervous temperament' is .53. Using the method of multiple correlation, the correlation between initial resistance and 'magnetic personality' and 'nervous temperament' taken together is .65.

It is needless to say that this study is not conclusive. That is obvious from the fact that so few cases have been used. It is highly suggestive, however, especially since the ratings of the judges show correlations indicating that they understood consistently what was meant by 'magnetic personality' and 'nervous temperament.' The correlation of .44 between estimates of 'magnetic personality' and initial resistance to the passage of an electric current is something of a surprise, and will possibly repay further investigation. As high a multiple correlation as .65 between initial resistance, and 'magnetic personality' and 'nervous temperament' combined, is still more of a surprise, and if corroborated will be quite significant in the study of personality.

MINOR STUDY FROM THE PSYCHOLOGICAL LABORATORY OF PRINCETON UNIVERSITY

Communicated by H. S. Langfeld

A Study of the Phenomenon of Apparent Movement

By Herbert G. Vogt and William Grant, Jr.

History. This experiment is a continuation of the well known work on apparent movement. The history of the experimentation on apparent movement and the so-called phi-phenomenon is given in recent articles by Dimmick, Higginson, and Helson.[1]

Following Wertheimer,[2] Dimmick in 1920 studied the phenomenon of the illusion of movement produced by non-moving stimuli. This investigator characterized the various movements as, "bi-membral," "uni-membral," and "intra-membral." In his analysis of the experience he reports that movement, optimal in all but a few cases, was reported by all *O*s for all colors and backgrounds when the instructions were "to characterize." Under the descriptive attitude there was noted a gray patch covering the space between the two members. This gray flash he reports is the psychological correlate of the perception of movement.[3]

Higginson in a later study attempted to resolve the opposed conclusions of Dimmick and Wertheimer.[4] Among the deductions which Higginson draws on the basis of his own experiments are the following: "It is impossible to set up a significant correlation between any one temporal interval and full-movement." "The fundamental assumption of Wertheimer's physiological explanation: namely, that the *phi-function* requires a determined and specific temporal course, does not stand experimental test." "The significance of a visual gray of a certain durational course, alleged to be the psychological correlate of visual movement, appears in the light of our experiments to have been exaggerated."[5]

The present study of this phenomenon was undertaken with a different apparatus and under somewhat new conditions.

Apparatus and Method. The apparatus used in this experiment was an adaptation of that described by Carmichael.[6] The slide which was used in the apparatus was arranged so that two circles of light 6 mm. in diam. and 5 cm. apart were exposed in immediate succession as the slide was dropped. The slide was actuated by gravity and, since there was very little friction, the speed was sufficiently constant for the purposes of the experiment. Wertheimer's optimal time interval of 60σ between the two exposures was used.[7] This time was determined by means of a tuning fork.

The following filters were used: red, green, red and green, blue and yellow. The colored filters were placed over the openings in the slide and

[1] F. L. Dimmick, An experimental study of visual movement and the phi-phenomenon, this Journal, 31, 1920, 317-320. G. DeV. Higginson, The visual apprehension of movement under successive retinal excitations, this Journal, 37, 1926, 63-75. H. Helson, The psychology of *Gestalt*, this Journal, 36, 1925, 498-519.

[2] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1921, 161-265.

[3] Dimmick, *op.cit.*, 330-332.

[4] Wertheimer's theory is physiological, and Dimmick's psychological.

[5] Higginson, *op.cit.*, 113.

[6] L. Carmichael, A device for the demonstration of apparent movement, this Journal, 36, 1925, 446-448. The slide used was an adaptation of that shown on the right hand side of figures IVa and IVb.

[7] Cf. Wertheimer, *op.cit.*, 178.

gave the desired colors to the circles of light as they were exposed, *i.e.* top red and bottom green; top green and bottom red; top blue and bottom yellow, top yellow and bottom blue. In another condition of the experiment the filters were not used and the dim yellow light from the box was exposed in two successive circles.

The experiment took place in a dark room in which only the fixation-point and the circles of light were visible to *O*. Eye-movements were partially controlled, and the field of vision restricted to a small area, just including the two stimulus dots, by requiring *O* to look through two black tubes, each one of which was 1 in. in diam. and 7 in. in length.[8] A fixation point was attached to the apparatus in the center of the space between the two dots. It consisted of a pin-hole of light. This light became visible when the slide was at its starting point with neither of the circles exposed and was extinguished automatically as the slide was dropped. There was, however, a sufficient interval between the extinguishing of the fixation point and the exposure of the first circle of light to eliminate the possibility of the former light influencing the result of the experiment.

Before every sitting each *O* was given the following instruction: "You will be presented with a series of illuminated dots of different colors and of different color combinations. Before each different presentation 5 preliminary exposures will be given you during which no report is requested; but rather, you are asked to attend to the experience at hand in order to familiarize yourself with what is taking place. After these preliminary exposures, a series of 5 similar ones will follow, after which an introspective account of what you have observed is desired. In order to enable you to fixate the field a small light will be given you before each exposure. This

TABLE I

RESULTS OF SERIES A AND B

O	No M	U′	U″	Bi	De	TPM	FM	Suc	Sim	Str	Intr
C	11	29	7	19	17	72	4	66	4	3	8
N	43	22	0	4	2	28	0	61	9	4	2
H	14	9	19	23	6	57	1	68	2	20	9
L	6	7	9	48	0	64	0	70	0	47	0
Gi	36	29	6	6	6	47	1	64	6	8	21
S	16	28	9	17	8	62	0	69	1	7	14
D	34	20	1	11	2	34	3	66	4	5	5
V	19	10	2	37	0	49	2	68	2	31	15
G	24	10	3	26	2	41	4	65	5	7	1
B	9	32	24	18	1	75	1	69	1	14	14
Totals	206	196	80	209	44	529	16	666	34	146	89
Av.	20.6	19.6	8.0	20.9	4.4	52.9	1.6	66.6	3.4	14.6	8.9
M.V.	8.6	10.6	4.7	11.8		13.2	1.0			7.8	5.7

KEY TO SYMBOLS

No M = No movement.
U′ = Uni-membral movement (movement of upper member down).
U″ = Uni-membral movement (movement of bottom member down).
Bi = Bi-membral movement (movement of both members down).
De = Delta movement (movement of bottom member up).
TPM = Total partial movements.
FM = Optimal or full movements.
Suc = Successive.
Sim = Simultaneous.
Str = Streaks.
Intr = Movement within circles, without other types of movement.

[8]By the use of this device eye-movements were much restricted. Incidental observations without the tubes suggest, as do certain introspective reports, that the eye-movement factor as a condition of the phi-phenomenon has been too much neglected. Restriction of eye-movement certainly seems to reduce the number of 'full movements' reported.

will be extinguished before the experiment begins. Should you fail to see this fixation point, notify the experimenter. In your introspective report give the nature of the experience."

Following the reading of the instructions the subjects adjusted the tubes to their eyes and the lights were extinguished.

The first series presented (A), consisting of 10 exposures, was as follows: (1) both discs green; (2) both discs red; (3) top disc green, bottom disc red; (4) top disc red, bottom disc green; (5) top disc blue, bottom disc yellow; (6) top disc yellow, bottom disc blue; (7) ordinary light. The second series (B), also with 10 exposures, was as follows: (1) ordinary light; (2) top disc yellow, bottom disc blue; (3) top disc blue, bottom disc yellow; (4) both discs green; (5) both discs red; (6) top disc red, bottom disc green; (7) top disc green, bottom disc red. A total of 140 were thus given to each subject, making 1400 for the group. Only the downward motion of the slide was exposed. When the slide was raised the stimuli were covered.

Observers. The *Os* who acted as subjects for this experiment Dr. H. S. Langfeld (L), Dr. Leonard Carmichael (C), Dr. W. S. Hulin (H), Dr. S. C. Dodd (D), Miss Marion Nagle (N), Mr. C. W. Bray (B), Mr. J. J. Gibson (Gi), Mr. H. Schlosberg (S), Mr. H. G. Vogt (V), and Mr. W. Grant (G).

Results. The two series of observations which are brought together in Table I were also tabulated separately, and the correlation between the rank order of the averages of each sort of report in each series was secured by means of the Pearson product moments method. The result showed a correlation of 0.945. This finding suggests that the sampling was adequate.

In the classification of the various types of movement found in this phenomenon, the terminology of Dimmick and Higginson has been followed. The attempt was made to analyze the constituent elements of the experience into "uni-membral," "bi-membral," "intra-membral," and "delta-movement." The coefficient of correlation was also calculated between the rank order of the subjects according to the number of experiences termed "streaks" and the rank order of the same subjects according to the experiences reported as "bi-membral movement." This correlation was high (+0.89). Most optimal movements were reported when the stimuli were of the same color. The *Os* reported 13 full-movements under these conditions, 5 full-movement experiences with both dots red, 5 with both dots green, and 3 when the color of both dots was of the direct yellowish light from the illuminated lamps. Only 3 experiences of full-movement were reported when the two stimuli were of different colors, 1 in the red-green, 2 in the blue-yellow.

The *Os* described the movements variously as: "streaks," "a connective bar of light," "a brightness between the light," "a filmy pathway," "a downward flash of light," "a connective film," "a vague film," "a flowing pathway." Frequently the lights "did not appear as dots, and there was a mere band of color which started at the top dot and moved downward all or part of the way." In the latter case a second band would commence after a stretch of darkness. The color of the experiences reported as streaks seemed to depend largely on the color of the stimulus circles. In most cases the streak was reported as somewhat lighter in hue, for example, as "a pale green," "a faint red," or "a dim yellow." In a few instances the *Os* reported a "grayish streak." The extent of the streak varied from a "slight downward flash" to "a moving pathway of light from the upper to the lower dot." Usually when the two differently colored stimuli were presented the accompanying streak was only of one color resembling either the upper or the lower dot. Two streaks of different color may be observed and H reported as follows: In the red-green series, "the green was displaced downward in a sort of flare and then directly below the green, but in nowise growing out of the green, there flashed a red streak down-

ward with increasing solidity and brightness ending in a red circle." The intra-membral movement was reported variously as "flaring," "quivering," or "shimmering" within either stimulus.

Conclusions. (1) Under the conditions of this experiment only a very few optimal movements were reported by the *O*s.

(2) The results show that the optimal movement experience when reported occurs more frequently when the two stimuli are of the same color than when they are of different colors.

(3) An experience of a streak between the two stimuli seems to be a consistent factor in the movement phenomenon. The color of this streak depends upon the color of the stimulus.[9]

[9]Cf. the results of G. D. Higginson, Apparent visual movement and the *Gestalt:* II, The effect upon visual movement of colored stimulus objects, *J. Exp. Psychol.*, 9, 1926, 239-252. In this paper, which appeared after the present study was ready for publication, Higginson presents certain results which are in agreement with those given here.

BOOK REVIEWS

G. Stanley Hall: A Biography of a Mind. By LORINE PRUETTE. New York, D. Appleton & Co., 1926 pp. xi, 267

Any biography of the founder of this JOURNAL and the teacher of so many of its readers is certain to be of interest. But this is no ordinary biography and must therefore be of interest to a much wider circle. As its subtitle indicates it is not so much an account of the life of G. Stanley Hall as it is a biography of his mind. This was rare good judgment. What he wrote needed no amplification, what he did had been recorded, but of the vicissitudes of his mind there was no depiction. This selection of material has resulted in a volume which is strikingly coherent and impressively intense. To the critically minded the wholesome influence of the modern movement in biographic technique as represented by Strachey, Maurois, and Bradford will be approvingly recognized.

The reviewer perused the book with intermingling waves of anger, resentment, sadness and delight, and closed it to wipe away a tear. It brought back to his mind, as it most certainly will to all Clark men of the old days, scenes he had long forgotten or would like to forget, memories which he cherishes, and a renewed consciousness of the sadness and isolation of that youthful spirited, grave, old man in the up-stairs study across the street where the light seemed always burning no matter how late the hour. One can never recall merely the Stanley Hall of the lecture room and the seminary. His mercurial behavior, the skill and the clumsiness with which he handled his student and faculty and scientific contacts makes Hall the man dominate the memories of those who knew him. With such memories it is a question if any one who was ever intimately associated with Hall is fitted either to write his biography or to review it. But it may be likewise questioned if any one who never had the compelling experience of his intimate acqaintance should attempt his biography.

This volume opens with a brief but excellent characterization of his life as a whole. The first chapter is centered about that youthful experience on a hill-top in western Massachusetts with which readers of *Adolescence* and the *Life and Confessions* are already familiar. It is rightly presented as the orienting feature of his life. He always believed that the experiences of youth shaped the course of maturity. Certainly it was true of his own life. Then comes the chapter on "Hall as a Puritan." He was a New Englander proud of his ancestry and in him the sternness, family pride, hardihood, reserve, and essential religiosity of the typical New Englander found expression. But Hall was not entirely true to type, and this is recognized in the chapter entitled "The Conscious Pagan." There was a frank enjoyment of life and a seeking after thrills which more than once pushed him into dubious if not actually dangerous situations. The author has tried to present this, many will think inadequately, but perhaps it is well that it should be minimized because the puritan in Hall always quickly reacted to it and sought to prevent the world from discovering it. Under the caption "Son and Lover" the story of his domestic relations is told. The beauty and tenderness of his love for his mother and the lifelong influence of that love is delicately revealed. The tragedies of his domestic life, the loss by death of his first wife and little daughter and the loss by mental disease of the second wife, are tenderly written. Those who knew him or his work know how thoroughly his was a pioneer soul. *Bahnbrechend* was one of his favorite terms. His writings and researches are here characterized as a work of 'horizon-breaking.' For the psychologist who is first of all interested in systems and interpretations the chapter quaintly entitled

"Cousinship to Peter Whiffle" will be long valued and frequently used. Hall had so often promised a huge book on emotions even up to the last weeks of his life it was but natural to suppose that the nearly completed manuscript would be found among his papers. Then, too, there was that startling course of lectures on the psychology of digestion and nutrition which seemed to indicate a brilliant collection of material deserving publication. But when his papers were examined all that could be found was a vast and bewilderingly chaotic mass of abstracts and random notes. The book on the emotions was not yet on paper in a form which any but Hall could organize and he had gone. The present reviewer was privileged to examine with the hope of publication all the notes used in those lectures on digestion and was forced to come regretfully to the conclusion that there was nothing publishable. Only from the notes of some careful student would it ever be possible to obtain the general scheme which he had in mind for those great topics. In this chapter Pruette has brought together and systematically presented the available notes on those two lecture courses. In other chapters the criticisms of other scientists are summarized, his religious thought is touched upon, and there is a happy application to himself of those measures of a man which he suggested in his *Life and Confessions*. Perhaps the best writing from a literary point of view is to be found in the last two chapters. His attitudes and interests and emphases after retirement from the duties of his chair and his presidency reveal how as an old man he looked at death and was not afraid. The last days are feelingly presented as the "Passing of the Playboy."

Where there is so much to praise one does not like to mention possible defects. Many will undoubtedly say that this is but a feminine view of Hall. But what if it is? There was much in Hall which a woman could best understand and appreciate. The harshness of Hall, the meanness, the selfishness, do not receive the emphasis which they should. While the pathos of his life is admirably presented it is perhaps too well done, because the reader will inevitably find the pathetic giving a sombre cast to all the rest. The humor in Hall, especially the delight in good jokes on himself, is lost sight of. It is also to be regretted that the author did not include a description of the physical appearance of the man. Perhaps it would not have been quite germane but a chapter on his physical appearance in the lecture room, on the street, with his automobile, running up the stairs in his house or in the university when late for a lecture, written by a hand so skilled would have been a lasting pleasure. Many, too, will wonder why the author thought it necessary to have a biography of G. Stanley Hall introduced by Carl Van Doren, a name but vaguely known if known at all to many who will read this book. Has the name of G. Stanley Hall been so far forgotten that it must be so re-introduced? But these are minor points in a book which is certain to live not only in the working libraries of his students and of all psychologists but also in the world's biographic literature for its interpretative and stylistic skill.

University of Oregon EDMUND S. CONKLIN

Behaviorism. By JOHN B. WATSON. New York, People's Institute Publishing Co., 1925, pp. 251.

"This volume," the author states in his Preface, "is designed for the use of undergraduates. While it is written in a somewhat free and easy style, every effort has been made to present facts in unmutilated form and to state theoretical positions with accuracy." It is the reviewer's judgment that the book is too airily off-hand and sketchy, as well as to dogmatic and combative, to be very useful as a textbook. Most of the chapters are much better adapted to the purpose which they originally served, as lectures delivered before a rather popular audience at The People's Institute.

In the first paragraph of Chapter I the author takes what some will think is an unfair advantage of the reader's natural human desire to be up-to-date, by indentifying behaviorism with the "new" psychology and all other psychology as the "old." Behaviorism is defined as holding that "the subject matter of human psychology is the behavior or activities of the human being," while "all schools of psychology except that of behaviorism claim that 'consciousness' is the subject matter of psychology." There is no suggestion of a psychology which would include both consciousness and behavior, or any intimation that there is any other brand of behaviorism than the author's own. This omission seems hardly in harmony with the author's avowed intent "to present the facts in unmutilated form" and "to state theoretical positions with accuracy."

In his discussion of the religious background of current introspective psychology, the author implies that the psychologist who would go beyond the study of behaviorism and concern himself with mental imagery is to be classed with the voodoo doctors or other believers in primitive superstition. The dualistic position of Wundt is characterized as "pseudo-science." Sensation, perception, image, desire, purpose, "and even thinking and emotion as they are subjectively defined," are in the usual militant fashion of behaviorism swept aside as "mediaeval conceptions."

Chapter II gives numerous concrete illustrations, interestingly presented, of how the behaviorist goes about his work. The methods of investigating the conditioning of responses and of stimuli are described. Chapters III and IV describe the human body under the two heads: (a) structures that make behavior possible, and (b) the rôle the glands play in everyday behavior.

Chapters V and VI answer the question whether there are any human instincts. Here the author denies not only the reality of instincts, but also the reality of innate "capacity, talent, temperament, mental constitution and characteristics." The evidence offered by the geneticists is thrown out of court on the ground they "are working under the banner of the old 'faculty' psychology. One need not give much weight to any of their present conclusions." "Give me a dozen healthy infants, well formed, and my own specified world to bring them up in, and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist, merchant, chief and, yes, even beggarman and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors." "Let us, then, forever lay the ghosts of inheritance of aptitudes, of 'mental' characteristics, of special aptitudes (not based upon favorable structure such as throat formation in singing, hand formation in playing, structurally sound eyes and ears, etc."

No factual data are presented in support of the above astonishingly dogmatic statements. One may well doubt whether such questions can be settled "forever" by a mere scratch of the pen. However, one can only admire the author's confidence in his particular brand of psychology, and if he can only show us that it is capable of supplying formulas which will manufacture any kind of genius out of any kind of human material, he should have little difficulty in quickly converting the whole world to behaviorism. Think how different our civilization and cultural status would be if our ten million or so dullest adults who have never been able to master eighth grade reading and arithmetic, or even to unravel a spoken sentence with more than thirty words and two clauses, had all been made into Aristotles, Shakespeares, Newtons, Raphaels, Leonardo da Vincis, Michel Angelos, Geothes, Beethovens, Darwins, Galtons, Clerk Maxwells, Pasteurs, and Einsteins!

Chapters VII and VIII are given over to the emotions. They are in the main an informal account of the author's own experiments and observations on the emotions of infants, and for this reason they are the best

chapters in the book. Factual data here have their one inning, in contrast to the more "verbal type of response" which characterizes the greater part of the book.

Habit formation and the psychology of learning are disposed of in the twenty pages of Chapter IX, "Our Manual Habits." Chapter X concerns "Talking and Thinking." The title is followed by the words, "which, when rightly understood, go far in breaking down the fiction that there is any such thing as mental life." For Watson, "what the psychologists have hitherto called thought is in short nothing but talking to ourselves." The arguments in support of this thesis are in the main a repetition of those the author has expressed in his other writings. In this chapter is the only specific citation of experimental literature which the reviewer has found in the entire volume—an article by Miss Thorson on the relation of tongue movements to internal speech. The next chapter "Do We Always Think in Words?" modifies somewhat the extreme position above stated. The thesis is that "one's total organization is brought into the process of thinking,"—that "manual and visceral organizations are operative in thinking even when no verbal responses are present," and that "we could still think in some sort of way even if we had no words." "We think and plan with the whole body." Even the acquisition of manual skills, we learn, is as much a matter of language training and visceral training as it is a matter of muscular training (p. 204). One can only regard it as unfortunate that we have so little experimental data on the possibilities and limitations of training for skill in the case of pathological subjects who have lost the power of speech or defective subjects (*e. g.* untrained deaf mutes) who have never acquired it. On the theory that verbalization is so important in the acquisition of motor skills it would seem a little difficult to explain the extraordinary feats of skill performed by many animals, such as deer, monkeys, birds, bees, etc. The author holds that infantile experiences are not repressed by the subconscious, as the Freudians think, but that they are lost simply because they have not been verbalized. This is an interesting speculation, but one that is difficult to prove or disprove. The reviewer indeed feels that a great many of the tenets of behaviorism, according to the gospel of Watson, resemble Freudian theories in their relative inaccessibility to the experimental approach.

The last Chapter, XII, is devoted to personality. In this realm, as in that of intelligence, the myriad of individual differences which we find are all acquired—"all healthy individuals . . . start out equal." The signers of the declaration of independence "would have been strictly accurate had the clause *'at birth'* been inserted after the word equal." Personality is defined as "the end product of our habit systems," including, of course, the visceral. There are thousands of these habit systems that go to make up a total personality. (Incidentally, 'attention' is merely the "complete dominance of any one habit system.") At thirty years of age the habit systems are pretty well fixed and personality made once for all. Personality is best diagnosed by the study of educational history, achievement records, recreational habits, and emotional responses. Psychological tests are of but limited value for this purpose at present. The chapter closes with an interesting discussion of our inferiorities, our susceptibility to flattery, our strife for dominance, infant carry-overs, etc.

Behaviorism will prove very useful as a reference for acquainting the student with one point of view in psychology. It is lively and challenging, with not a dull page. The language is intelligible even to the average college freshman or the moderately educated layman—something which can be said of but few books on psychology. It can hardly prove serviceable, however, as a general introduction to the science of psychology; it is much too confined to the limited (if important) field which the author has himself cultivated. It gives the reader hardly a hint that work of any im-

portance has been done by any other psychologist, although vague reference is occasionally made to investigations by Lashley.

In view of the extraordinary theoretical and practical importance of the experimental work which Dr. Watson inaugurated at John Hopkins, the reviewer lays down this book with a feeling of profound regret that an investigator so original and resourceful should have been lost to scientific psychology. Had his fruitful experimental work continued he would now in all probability be giving us experimental data of untold significance on mental development and learning instead of verbal fireworks on the merits of behaviorism and the idiocy of other psychological viewpoints.

Stanford University LEWIS M. TERMAN

Science and the Modern World. By A. N. WHITEHEAD. New York, Macmillan Co., 1925, pp. xi, 292.

A review of this book should attract new readers to it, and should at the same time to prepare them for an arduous experience.

There is little question that Whitehead's work is of first class importance. His studies are among those which give a new direction to scientific and philosophic thought. He is remarkable in being not only a most able mathematical physicist but also a serious student of European culture and a lover of the humanities. His theory suggests new prospects for the future of all the sciences, and is boldly constructive.

But there are also some intimidating facts about this book. Large parts of it are written with a most difficult abstractness; the sequence of the argument is not always clear; a fairly large new vocabulary is employed; whole paragraphs and pages, whole chapters, are as oracular and as difficult to grasp as if they came from Hegel. It would be a fortunate reader who did not experience frequent attacks of discouragement and impatience with so hard an exposition, and who did not find some haziness even in his final understanding of Whitehead's theory. Fully half the book is laborious and difficult, nevertheless the labor is profitable. Most readers will feel that Whitehead's thinking is difficult to follow largely because it is in fact an attempt to re-shape the fundamental concepts in modern science. Part of the reader's effort is like that of climbing; and at least part of the book's cloudiness is such as one is likely to find in the view from a height.

Whitehead attacks the materialistic postulates which have ruled in the science of the last three centuries, and sketches an alternative conception of nature, employing a different set of ultimate concepts. He gives grounds for discarding the traditional belief that physical reality can be analysed into substance units of some sort, and that all natural happenings are merely mechanical rearrangements of these. Mutually independent units are abstractions which have been justified heretofore by their usefulness in the development of science, but which are now proving clearly inadequate for the more complete understanding of nature. In their place the primary concepts which Whitehead adopts are those of interdependent events, unlimited fields of influence, organic processes of various degrees of complexity and endurance, "eternal objects" such as the abstract qualities of sensory experience, and an underlying "substantial activity."

Mechanism he rejects everywhere in nature. This was an abstract theoretical construction which earlier scientists developed along with their materialism, and which for a time had a great pragmatic value. But mechanism is quite undeserving of the metaphysical prestige it has acquired even among biologists and psychologists. Configurations and unitary patterns in natural events are given as much objective importance as an *Gestalt* psychologist could wish. The theory of organic evolution is extended in the widest possible manner: it becomes an essential part of every natural science. The modern mathematics of functions and the new relativity physics are drawn upon for evidence that we must revise our beliefs as

to the nature of concrete reality. Individual human experiences are brought into a general theory of nature which considers psychological occurrences to be only special cases of a certain fundamental type of event. Vitalism is discarded along with dualism and mechanism and mechanical determinism. The individuality of every part of reality is emphasized, and also the half fictitious character of the scientific generalizations which leave this individuality out of account and deal merely with certain abstracts aspects of things. The full reality itself must be otherwise conceived.

Whitehead writes primarily as a student of physical science, but hardly less as a man who is sensitive to the esthetic and the humanitarian interests of life. In arguing that we ought not to attribute any importance to materialism and mechanism as ultimate truths about nature, he accordingly employs two different sets of observations. There is the fact that a large part of our actual experiences, especially our enjoyments and our valuings, are so obviously unassimilated by that kind of metaphysics. There has been a genuine conflict between the views which thoughtful men have believed science required them to adopt and the other aspects of human life which find expression especially in the arts. To some extent the dominance of the scientific doctrines has been an injurious factor in the social and cultural development of our time. But, quite independently of all this, the recent increase of knowledge in physical science itself has proved the necessity for a new conception of reality. It was Whitehead's work in mathematical physics which convinced him of the final defectiveness of the classical mechanism-theory. A form of relativity physics must be adopted instead, not quite of the Einstein variety, but one with just as difficult a doctrine of space-time and the continuum of events, and just as destructive of the beliefs we have been accustomed to accept from the physicists.

Not substantial particles, not even units such as electrons, but events or 'occasions' are the ultimate concrete facts. Reality itself is indeed one event. By abstraction, however, we can discriminate lesser events which can be considered as its components. Certain trains of these events are, again abstractly, the more or less enduring things which are given so much importance in our ordinary thought and our traditional science. Here the essential fact is that a certain pattern is reproduced in the series, a relatively simple pattern in the case of an electron, and a complex one in the case of a human being. But a relocation of independent pieces is not the final truth about any real occurrence. "We must start with the event as the ultimate unit of natural occurrence." "The event is the unit of things real." Every occurrence, however, is dependent on its relations with all others. The relations of one event to others "are constitutive of what the event is in itself;" they are functional in the mathematical sense, and are "internal."

Even a unitary event is still capable of a different analysis. Here the task of the reviewer becomes difficult. Whitehead considers that into every event there enters a set of factors which he terms "eternal objects," and which he illustrates as "colors, sounds, scents, geometrical characters," and "relationships of these objects among themselves." Each of these remains identical and unaltered, however many events may include it at one time or another; but each of them stands in a set of relationships to all the others. The system of these eternal objects constitutes the realm of possibility. An actual occurrence is a certain finite "prehension" from this system; and, in the large, concrete nature, the real world, is such a selection. "The metaphysical status of an eternal object is that of a possibility for an actuality." With some of these objects, as united into an actual event, we have direct acquaintance. "The psychological field exhibits what an event is in itself." "The private psychological field is merely the event considered from its own standpoint." Individual human lives consist of a set of occurrences, among which some are cognitive; and the possibility of cognition depends on this, that the eternal objects which enter into events are related

in certain ways to other eternal objects which compose other events—as for instance a certain color in a particular experience is related to the constituents of those other events which we call a candle burning at the other side of the room. Only because events are systematically inter-related and interdependent can experience be cognitive at all.

Finally the analysis of events requires a still more difficult concept, and one which receives too brief a treatment in Whitehead's book. There is a "substantial activity" which constitutes each real event by unifying the particular set of eternal objects which compose it. This activity is "not an entity in the sense in which occasions or eternal objects are entities." "It is Spinoza's one infinite substance."

The concept of a pattern and organization of events is of special importance, and Whitehead is already being quoted in support of *Gestalt* psychology. Physical science, he believes, has not less need than biology for the concept of organisms, in the sense of patterns of events which are self-repeating and self-maintaining in their interaction with their environment. The discontinuity which recent physics apparently requires in some of its primary objects is also provided, he suggests, in the repetition of simple pattern units, giving a series of wholes which are discrete even though the process within each unit is continuous.

As any part of a human organism is affected by its relations with all the other parts, so similarly any part of any enduring physical structure should be conceived as affected by its place in the larger pattern. It is, he thinks, an unfounded and mistaken supposition that the physical components of an animal body are exactly similar in behavior to those in other environments. This does not give any support to vitalism; but it does lead Whitehead to leave a place in his theory for the possibility that a process in one organism can be directly determined by a certain process in another. He seems to hesitate a little to declare for telepathy, for he avoids using the term in his reference to the subject; but he clearly implies that his organic theory of nature could allow for some phenomena of that sort.

In contrast to the hard sections of the book are the very readable first chapters which sketch the rise and development of science during the last three centuries, and the last chapters on the relation between religion and science. Whitehead draws upon an enviably wide acquaintance with the history of European culture; but one is left somewhat doubtful by his analysis of the psychological origins of the growth of scientific knowledge during the 16th century. Greek tragedy, Roman law, and scholastic theology may have had the essential preparatory importance which he attributes to them, but the argument is at best too brief to be convincing. Psychologists are not likely to approve his repeated remarks that these influences finally produced in many Europeans an "instinctive" belief in the set of doctrines which underlie modern science. In a later chapter, when his reference is really to instincts, he speaks of "the mere impulse of the various senses." But the somewhat casual psychology suggested by these phrases does not after all affect the theory presented in the book, and should be mentioned, even here, only in passing.

Amherst College CHARLES H. TOLL

Science, Religion, and Reality. By LORD BALFOUR, B. MALINOWSKI, C. SINGER, A. ALIOTTA, A. S. EDDINGTON, J. NEEDHAM, J. W. OMAN, W. BROWN, C. C. J. WEBB, and W. R. INGE. New York, The Macmillan Co., 1925, pp. 396.

Not only timely in the light of much recent discussion and legislation on matters of religious and scientific interpretation, but also important for all time in the cross-section that it cuts out of the stream of thought through the ages, is this delightfully conceived series of essays. The editor

(Needham) stipulated that the contributions "should be solid, but not too technical for the general reader," and that they "should not be directly apologetic in tendency." As Dean Inge concludes, the essays have very well met the specifications. They maintain a high level of scholarship revealed not only by ample historical citations but by cautious judgments in the face of controversial opinion or of insufficiently proved points under investigation. Besides two of the essays in particular, one on "The Domain of Physical Science" and the concluding one, display a fine sense of humor as well.

In the introductory essay Lord Balfour persistently searches for the point or area where conflict between science and religion might arise. Collisions do not come so much from the thought processes involved in both as from the emotional attitude engendered, but even when the sheer facts of science jar on our religious sensibilities we must not forget that the glory of the heavens which science reveals is itself born of our being. "If we are nothing, they are nothing." There are some, however, who regard the contradictions not emotional but factual and intellectual. In that case the Bible is often misunderstood as "a textbook of cosmology." The happiest solution of all these problems lies "provisionally in an unresolved dualism;" the world of science and the spiritual world are fostered by a double outlook on our experience; and both are real.

Under the title, "Magic, Science and Religion," the first writer gives a genetic account of the growth of magic and religion among primitive peoples. They are "not merely a doctrine or a philosophy . . . but a special mode of behaviour, a pragmatic attitude built up of reason, feeling, and will alike." Through intimate study of the Melanesians of the Trobriand Archipelago he concludes that the three factors make a three-cornered constellation. Where knowledge is inadequate, primitive emotions and instinctive reactions demand a satisfactory outlet and thus become crystalized into permanent traditional forms in magical rites and beliefs which in turn are fixed by religious faith. Singer in the next contribution traces the historical relations of religion and science. He begins by pointing out that the Greeks made large contributions to science and philosophy without possessing a canon of sacred literature. At another place he makes the startling statement that the Renaissance meant nothing more than a revival of *ancient* science. Roger Bacon was not regarded as a heretic by his contemporaries. It was later that conflicts began to arise. Step by step the author traces the progress of science until the present day. There are then four ways in which the man of science can envisage the interrelationship: he can use his science in the laboratory and study and his religion outside of these boundaries; he can disregard his religious attitude altogether; he can mystically separate the internal from the external experience; and in pantheistic fashion he can square himself with determinism by regarding his soul beyond his ken and yet in transcendental harmony with the world-soul.

The following essay in scholarly analyses outlines the progress of philosophical thought in connection with religion. The intellectual element is throughout an integral part of religion. Although fulfilling different functions in life, religion and science together approach the reality of things. Only an old-fashioned faculty psychology would permit of a relegation of religion to the emotional life and of science to the intellectual life of mankind. The essay on the domain of physical science, by witty illustration as well as by learned discourse, shows the limitations of the 'interventions' of science. Piece by piece the picture-puzzle is put together. Beyond this the scientist can not go. The philosopher is privileged to guess and the man of religion is bound to believe what the picture will look like when completed. Then there is a paper on mechanistic biology and the religious consciousness. Again in historical array the development of biological

thought is presented. While" the bio-chemist and the bio-physicist, therefore, can and must be thoroughgoing mechanists," they need not hesitate to affirm on other grounds the existence of religious data. Contrasted with this discussion a paper follows on the sphere of religion wherein the author contends that religion is not reduceable to any mental feeling or attitude but to an outward reference that is dependent in turn on the assumption of a supernatural reality. Just as the natural world derives its reality from factors outside of the evidences of the senses, so the supernatural world, without sensory appeal, is grounded on values that have a similar origin.

In contrast to this the essay on religion and psychology takes the view that religion is a state of mind, a mental attitude with which we face existence in terms of cognition, feeling, and conation. This attitude the author analyses in regard to time and personality. The final essay in the series traces the larger aspects of science, Christianity and modern civilization. Science itself can not produce the values with which the achievements of science are appraised. These are the products of a Christianized world. Christianity in its place has learned much from the achievements of science, and has been made over by them.

The concluding summary by Dean Inge is a masterful writing. Anthropological and historical inquiry are elucidating but limited in scope. "It tells us nothing about Newton to know that he once had a tail. . . . We do not care much how it [religion] began; we want to understand it as it is or may be." Furthermore, while science can never be free from evolution, its main purpose is not to derive these values. Religion, which is not just another field of activity delimited from that of science, studies the essential values of goodness, truth, and beauty. The mystic attitude is not more emotional than intellectual, but a combination of mental abilities in which some individuals are more and others are less gifted.

The whole array of essays is a noteworthy achievement. At a time when scientists are accused of irreligion the writers all reveal not only a wholesome attitude toward the everlasting verities of life but a rational aspect to their convictions. There are minor differences of opinion in the explanations of things, but there is always a choice available even for the sensitive reader. The reviewer wishes that the volume find wide publicity. The format is attractive.

University of Iowa CHRISTIAN A. RUCKMICK

Logic: Part III. The Logical Foundations of Science. By W. E. JOHNSON. Cambridge (England) University Press, 1924, pp. xxxvi, 192.

The two preceding parts of this work deal with topics relevant to Formal Logic. In this volume the author is concerned chiefly with scientific or causal laws and with problematic induction. By 'problematic induction' is intended what is commonly meant by 'induction' merely; but Johnson recognizes other kinds of induction (see Part II).

At the beginning of the discussion the author distinguishes two kinds of universal propositions: those which express universals of fact, and those which express universals of law (chap. I, Fact and Law). If I say, 'Everything that has the character p has the character q,' I express a universal of fact; but if I say, 'With regard to every thing, if it had the character p it would have the character q,' I express a universal of law. The first proposition follows from the second but the second does not follow from the first—a universal of law entails a corresponding universal of fact, but not conversely. Universals of law, Mr. Johnson says, express *nomic* necessity (chap. I, 3d,). It is often held that any law of nature is simply a factual universal. But it is to be noted that we often make such judgments as, 'If this book were released it would fall,' which seem to be inferred from some law of nature, and this proposition does not follow from any merely factual universal like 'All bodies of a certain kind which are released fall.'

Moreover, in the case of universals which are certainly merely factual, such as 'Every book on this table is green,' we do not infer, for example, that 'if *that* book were on this table it would be green' or 'if that red book were on this table it would be green.' Nomic necessity, Johnson holds, is to be distinguished from factual universality.

As a deductive relation, we may have 'p implies q, and p, therefore q,' which is demonstrative; as an inductive relation, we may have 'p implies q, and q, therefore p,' which is problematic (chap. II). Induction is concerned with instantial propositions as evidence for the truth of a universal from which these instantial propositions follow. Now the force of the evidence for a universal is only indirectly dependent upon the number of examined instances. Indeed it is possible that the number of examined instances might be increased indefinitely without increasing the probability of a generalization based on these instances. It is only when the examined instances differ and agree in relevant ways that the probability of the generalization is increased. A new instance strengthens the evidence for a generalization provided it satisfies the generalization and also lacks some character, which all the other instances have in common. The evidence is further increased by the occurrence of two such characters provided the occurrence of neither of the characters necessitates the occurrence of the other, either logically or through some law of nature. It is not the number of characters but the number of independent characters that is important.

The author employs the term *eduction* to denote inference from instances to instances (chap. IV). Inference from a set of instantial data to some unexamined case is always more certain than inference to the corresponding universal, for the unexamined instance follows from the universal, whereas it might be the case that the universal is false and the instantial proposition true. Now it may happen that all of the evidential data are favorable to the truth of the proposition to be inferred, or it may happen that some of the evidence is favorable and some is unfavorable. The former case leads to an inductive generalization which is universal and the latter to an inductive generalization which is class-fractional (p. 47).

This is the kind of thing with which Johnson is concerned. He has also a discussion of the mind-body relation (chap. VIII) in which he applies his analysis of substance and causality.

Harvard University C. H. LANGFORD

Psychology and Education. By R. M. OGDEN. New York, Harcourt, Brace & Co., 1926, pp. ix, 364.

Since the advent of configurationism, especially since the work done by Köhler, most students of education have felt the need of applying the concepts the *Gestalt* school has evolved to some of their problems. Koffka's *The Growth of Mind* was essentially genetic; articles in different journals have been concerned with some one phase or another; no one book had been issued which purported to carry *Gestalt* to education.

The advent of this book by Ogden is an attempt to meet this need. Dedicated to Kurt Koffka, it is a wholesome attempt to read configurationism into most of the problems of educational psychology and thus show a *via media* for avoiding the pitfalls of both behaviorism and teleology. In his preface Ogden expresses the belief that he has been able "to give for the first time to the study of education a psychological background which commends itself as an orderly and systematic development of scientific principles derived from factual observation rather than from neurological tautologies."

The result is a fairly comprehensive study of the field covered in a course in educational psychology, a field which he has enriched by giving the results of his nine years of teaching such a course, and a critical survey

of the experimental work bearing on the point of view he adopts. To him mind is not a passive thing transformed into a bundle of habits and pathways, but rather a functioning force capable of creative efforts and achievements, the powers of which are realized only when education liberates the child to follow his own will and intention.

Man's original nature is discussed in the first seven chapters in a manner that shows gleanings from every available source to substantiate the claim, which Ogden consistently makes, that original behavior of any form shows three characteristics: adjustment, differential sensitivity, and variation according to the needs of the organism. Sucking he analyses in detail to illustrate his contention. To him the most primitive form of behavior is an undifferentiated response of the whole organism. In its more developed form, tropistic, reflex, and instinctive behavior are seen centered around some object or situation, and showing always the features of avoidance or acceptance.

A lengthy discussion of the instincts is made on the general basis of River's classification into individual, sexual, and social instincts. Arguments for their existence are based on work done on animals as well as human beings, and apparently equal weight is given to each. The *Gestalt* influence is again apparent in his addition of adornment and cleanliness to the individual instincts, in his distinction between gradual and saltatory development under the sex instinct, and in a few pages devoted to integration under the social instincts. Imitation and play he discusses separately contending that both involve no unique patterns of behavior, yet show their original natures in their attitudes. "It is not the particular action of the others jaws which is imitated, but yawning in a generic sense" (p. 99).

What is really the second part of his book is prefaced by a chapter on "Situation and Experience," where an attempt is made to show the functional unity at the basis of both behavior and situation which makes all experience possible. "Just as the behaviour of an organism is always somehow patterned . . . so the situation which is the counterpart of behavior is also patterned" (p. 109). "The situation . . . is something which participates as a member of the organic pattern of response (p. 111).

Chapters on Perception, Affection, and Sensation follow. Here the influence of the *Gestalt* school is most apparent, although fused with it —especially in the chapter on Affection—the work of behaviorism is pronounced. Perception is not "a complex of sensory experience and associated imagery" but rather "the experience of a closed circuit of events—a satisfying experience—the satisfaction being found by finding an end appropriate to the beginning" (pp. 127 f.). Affection like sensation depends upon the dispositional ground from which a perception emerges. The educational application of these is well thought out in a study of the acquisition of skill under perception, in a repetition of the accepted value of emotion for learning under affection, and in a genetic development of the sensory qualities in the different sense departments.

The last half of the book is given to chapters on memory with a classification of the main laws and some classical experiments bearing on them, on learning in both the process and patterns, and on thinking, reasoning and the measurement of intelligence. Koffka's classification of the process of learning—particularization, differentiation, assimilation, gradation, and redefinition—he adopts, using Köhler's experiments on the chimpanzee to bear out his claim. The patterns of learning comprise for the most part a genetic study of word formation; thinking and reasoning duplicate Creighton's *Logic*; the measurement of intelligence is criticized after the manner of Boring's "What the Tests Test." Yet his using of others' material is only a gleaning from varied sources to substantiate his basic claim that the organism acts as a unified whole to a unified situation, the contours of which are dependent upon the temporary, internal, organic condition.

The book as a whole is a distinct contribution to education, and ought to serve as a good text for students in educational psychology with previous training in psychology. Its defects are of a minor kind; many of the sentences are unnecessarily involved; thoughts that could be expressed in one clear sentence are often juggled about in such a way that one has to read the paragraph many times before getting the meaning; references are mentioned without being indicated; and selections are chosen which confuse rather than simplify the point being discussed. Yet, despite these faults, the book presents a new point of view in a consistent manner, and offers a solution to many of the perplexing problems in education.

Harvard University HERBERT MOORES

Experimental Psychology. By MARY COLLINS and JAMES DREVER. New York, E. P. Dutton, 1926, pp. viii, 315, 27 diagrams.

This book is a combination introductory laboratory manual and textbook of general psychology; tending toward the former in its subject matter, and toward the latter in its popular mode of presentation. Its preface informs the reader that it is meant to serve as an introduction to more advanced textbooks of experimental psychology, but unfortunately the authors seriously detract from its usefulness by not treating adequately their references and bibliography. Only an occasional reference is given to current psychological periodicals despite the fact that the most recent and usually the most significant information is to be found in them rather than in textbooks. All the references that are given are without the initials of the author, the name of the publisher, and the date of publication.

An Introduction of 25 pages gives a brief discussion of the evolution of experimental psychology, the nature of the psychological experiment, the value and application of psychophysical methods, and the statistical treatment of data. This introduction would be improved if reference were made to the stimulus error, as well as to the uncritical use of the normal curve of error.

In the consideration of the senses, Vision is given the customary major portion of space. Occasionally there seems to be an attempt to 'impress' the reader by the use of imposing terms. Together with the three well known color theories it is surprising to find an exposition of the Eldridge-Green theory. In view of the fact that this theory has been so roundly criticized by such men as Parsons and Sherrington it is questionable whether it should be included. Hearing is orthodox except that a disproportionate amount of space is devoted to Watt's theories of consonance and hearing. The Chapter on Cutaneous Sensations is poorly done. Head's theory of 'protopathic' and 'epicritic' sensibility is accepted *in toto*, without mention being made of the experimental results opposed to it. Neither is Abbott's work on physiological zero referred to. Taste and Smell, and Movement and other Organic Sensations are, quite properly, disposed of briefly.

Perception is given only 32 pages. The authors believe firmly in *Gestalt* psychology, and assert that it "bids fair to revolutionize the psychology of perception" (p. 107). From their mention of the valiant fight made by the *Gestalt* psychologists against the error of the 'bundle hypothesis,' the reviewer infers that the authors hold to the doubtful assumption that there are still numerous adherents of strict sensationalistic psychology, and that the 'bundle hypothesis' exists as a controversial issue. The statement that the *Gestalt* psychologists have shown "how, and in what sense, perception is always perception of a single object, that is, of a whole" (p. 108), is ambiguous. What is meant by a 'single object' or a 'whole'? In the next chapter, on Attention, the authors state "we regard a house as a unit, and as a single object; yet it consists of an infinite number of parts, windows, doors, chimneys, etc., each in themselves units. The 'singleness' of the object, therefore, varies according to the purpose at hand.

An object may be one single thing or it may be a number of things grouped together and regarded as a single object" (p. 140). These two statements are incongruous; it is difficult to reconcile them. For a more critical evaluation of the *Gestalt* doctrines, sophisticated readers of Collins and Drever can now consult the articles by Helson which have recently appeared in this JOURNAL.

The authors seem to have included nothing in their chapter on Attention that has appeared since Titchener's *Elementary Psychology of Feeling and Attention*, 1908, in spite of the fact that a number of important experimental articles on the subject have been published since then. The range of apprehension, for example, can no longer, in view of the work of Fernberger and of Dallenbach, be said to be 4 or 5.

The section on Action is brief and on the whole satisfactory. Work and Fatigue, and Suggestion and Suggestibility are fields in which the authors are competent. The chapters of Feeling and Emotion, Imagery and Association, Learning and Memory are adequate. Imagination and the Higher Thought Processes, Language and its Development, and Mental Testing conclude the main body of the book. Considered as brief treatments, there is little fault to be found with these chapters.

From a pedagogical point of view the book has certain virtues. For the most part it is written in a clear, simple, and entertaining style. It stresses and demonstrates the importance of the experimental method; it mentions and describes the chief methods and apparatuses available for the investigation of various psychological problems; and it gives a simple and fairly adequate account of the result in various divisions of the experimental field. For these reasons the book should serve fairly well to assist the average first year laboratory student in becoming acquainted with the scope and technique of his subject.

Harvard University HARRY R. DESILVA

Experimente mit Raphael Schermann. By OSKAR FISCHER. Berlin and Vienna, Urban Schwartzenberg, 1924, pp. 200.

Those who remember the furore that Raphael Schermann's demonstrations in New York some years ago made in psycho-medical circles will be interested in this monograph by Professor Fischer of the German University in Prague.

Dr. Fischer who is a psychiatrist seems to have obtained some remarkable results with this extraordinary man who was able to characterize a person he had never seen from the handwriting, although not after the fashion of the so-called graphologists. Schermann is alleged to have told details about the subject's history, about his incipient ailments, about his circumstances, etc. Many are the experiments which were carried out, and the results cover a wide variety of phenomena, bordering on the veridical.

Perhaps the most puzzling achievement of Schermann is the imitation of the handwriting of a person that he had never seen. Dr. Fischer would visualize one of his acquaintances, and Schermann would produce a fair specimen of that person's chirography.

In all these experiments there were of course a number of failures, but the proportion as a whole is as 27% failures to 65% successes with 8% of uncertain responses.

What conclusion may be gained from these experiments? The author is forced to postulate an "extra-sensual transfer of psychic processes among human beings," which is mediated through some sort of cerebral energy. The *modus operandi* of this process remains a mystery. Furthermore, however, since Schermann could describe conditions which were not thought of by the experimenter—details of the past and present milieu which were not even known to him—when he was concentrating upon the

external appearance of the absent individual, there is no alternative but to ascribe to Schermann the gift of clairvoyance or cryptaesthesia. Fischer discusses the question that possibly the transfer is of the subconscious, but counters this by the fact that he was absolutely ignorant of many of the particulars which the observer brought to light.

Clairvoyance or, as it is more technically known, *cryptaesthesia* would be explained on the basis of irradiation of a supra-sensual energy between object and organism. It is curious that although Schermann could give much information (mood of the writer, number of pages) about a letter written, only by fingering it, he could not guess one word of the contents. The author surmises that possibly a certain psychic energy, typical of each individual, attaches itself to the writing, and it is through this energy that Schermann is able to describe the writer.

For a reviewer to attempt to criticize a series of experiments so inaccessible would be folly; and there will always be the tendency on the part of the critical to say, "Since I was not present, I cannot accept the evidence," —an attitude of which the author is keenly conscious. To the reviewer's mind, it does seem an impossible feat for a human being to be able to tell that some one has married for money or is in straitened circumstances, etc., without having had the slightest information about him. Such omniscience would imply that all the experiences of a person, his past motives, and so on, are clinging to him not only in the form of effects (which can readily be understood), but also in their original configuration, so that when his external appearance is visualized, these in some way reveal themselves to the clairvoyant.

These experiments, it may be said, were conducted before Schermann visited the United States; and after 1918. Dr. Fischer was not able to sustain the coöperation of the observer, who had taken an aversion to the experiments. But there is the rub. It is so disconcerting to have to discontinue experimentation because of the attitude of the very person on whom all depends; and here is the great drawback of scientific investigation of the occult. In the physical sciences, results once achieved are held and allow of the possibility of reproduction, duplication, and expansion. Subsequent workers can always build on the foundations of their predecessors. Until we get to that stage in the region of the occult, investigators will always encounter the justifiable skepticism of those who are unable to verify the conclusions because of the resistance of the medium or other person crucial to the test.

Cambridge, Mass. A. A. ROBACK

The Ways of the Mind. By H. F. ADAMS. New York, Charles Scribner's Sons, 1925, pp. 336.

The chapters in this book are what one has come to expect in an elementary text in psychology. Emphasis, however, has been placed on the dynamic side as shown by separate chapters on Interest, Desire, Will, and Motives and Incentives. A promise of the Preface to give separate chapters to Originality and Intuition is not fulfilled; the first of these two topics is considered only in the discussion on Thinking, and the latter is entirely neglected—it is not even mentioned in the Index.

Though reproducing a large number of good cuts, 12 of which are taken from Howell's *Textbook of Physiology*, Adams has presented much detail that he does not make use of in the text. The elementary nature of the book appears in the over-simplification of the treatment of some of the psychological processes, arbitrary presentation of certain theories without indicating that there are rival theories, and a marked tendency to define.

The tendency to define has lead Adams into many inconsistencies. For example: association is described as "the most fundamental of all psychological principles" (81), and later he says, "it would appear that in-

stead of attention being the unit of association the associative process is the unit of attention" (150). Incentives are "external conditions" which affect response (298). Primary attention "cannot be interesting, for it is a mere passive awareness without meaning, purpose, or future reference;" "attention both analyzes and synthesizes the parts of which a complex whole is made" (124). "Images are necessary in all thinking" (232). "Pleasure results from the activity of well nourished nerve cells (194). Strangely enough 'assent' is postulated as essential to all movements; yet "in the case of automatic, reflex, instinctive, and random movements 'assent' is understood, and the movement takes place in a purely mechanial way" (274).

The verbalism of the text is best seen perhaps in the chapter on Interest. A sample passage begins, "Interest, we see, is essentially a mental examination of objects . . . in which the self is not completely identified." Emotions "are called out by objects and situations having immediate and personal value" (218); but "Value . . . [is] an ideal as compounded with emotion."

The subject matter in most of the twenty-one chapters is presented in one, two, three order; consequently they are easy to outline. The chapters on the Nervous System, Sensation, Memory, Attention, Perception, and Fatigue are clear and adequate for such a book. Adams, however, seems most occupied with the question of the nature of interest. Textbooks of this kind make one question whether it is worth while to keep on re-writing the older theoretical psychology for students who should be getting a fresh glance at the living science.

Louisiana State University PAUL C. YOUNG

Arthur Schopenhauer als Psychologe. By RICHARD HOHENEMSER. Leipzig, J. A. Barth, 1924, pp. 438.

In a system of thought so highly integrated as is Schopenhauer's philosophy it is no easy matter to find the catalytic agent that will precipitate out effectively and clearly the psychological elements that are dissolved therein. The author seems happily to have discovered the agent. In a very comprehensive way he not only analyzes but expertly weighs the contributions made by Schopenhauer to modern psychology. The four chapters deal with each of the larger subdivisions: the doctrine of recognition, including apprehension and comprehension, judgment, association, memory, the comic, delirium, and dream; the doctrine of will which involves questions of instinct, mental inheritance, and magic; the doctrine of beauty as applied to architecture, painting, poetry, and music; and the doctrine of moral relations, with especial reference to the freedom of the will, to conscience, punishment, and the activities of society and the state.

Because Hume first gave a thorough-going mental interpretation to human understanding, he is recognized as the first great psychologist. Then comes Kant. He proved the *a priori* character not only of all perceptual activity but also of conation and pointed out their manifestation in art and ethics. The contributions of Fichte, Schelling, and Hegel are of little account psychologically since they either lost their psychological bearings in their search for the metaphysical or else followed courses that were parallel to but far removed from psychology.

But Schopenhauer hewed close to the phrase, "the world is my idea." Perceptual presentations were conditioned by objective data and by sensory limitations, but they were undoubtedly also at the mercy of the past experience of the *self*. At once we notice the dynamic power of conation and its influence on the mental life. Man is not to be regarded as a microcosmos, he thinks, but the world at large can be better understood as a macranthropos. So like Descartes and Berkeley before him, Schopenhauer studied in detail the processes and principles of human perception and thought in order the more adequately to comprehend the world at large.

In the reviewer's judgment, however, the author does not show clearly the danger of anthropomorphizing the universe without seriously trespassing on the intrinsic meaning of such concepts as *conation* or *will*. The author quite rightly defends Schopenhauer's notion of unrecognized "motives" in instinctive action, but fails to account for the magnification of the effort or drive of the human mind into worldly or universal dimensions. While Schopenhauer did psychology good service by portraying passive observation as active and dynamic at a time when logic still ruled with an iron hand in psychology, he seems to have overreached himself when he tried to convert all mental processes, and perhaps physical events as well, into a supreme voluntarism—even in the face of an outworn intellectualism.

The monograph is well organized and the material is handled with sympathetic insight into Schopenhauer's philosophy and with keen understanding of many modern tendencies. Where many treatises of this type become arid by meandering through plans of abstract phrases, the author sweetens his task with apt illustration and epigram.

University of Iowa CHRISTIAN A. RUCKMICK

Emotion and Insanity. By S. THALBITZER, translated by M. G. Beard. The International Library of Psychology, Philosophy and Scientific Method. New York, Harcourt, Brace and Co., 1926, pp. 127.

This book consists of a preface by Harold Hoeffding and four chapters devoted respectively to Introduction, Psychology, Psychiatry and Psychology, and Physiology.

The author treats the subject comprehensively but not exhaustively. The book is written in a style that can be understood by the average student of psychology.

The chief contribution to the subject is found in the chapter of Psychiatry and Psychology, where the author gives a lucid and comprehensive description of the various mental types found in insanity.

Professor Hoeffding very ably depicts the merits of the chapter in his preface where he says: "In two respects in particular the author has, I feel, made important contributions to psychology. In the first place there is the general conclusion at which he has been enabled to arrive by a study of the manic-depressive psychosis. He shows that within this we find every kind of feeling represented, even the most complicated, and on a more elaborate scale, so that each particular feature appears with greater clearness. We thus become aware of features which are not easily discovered in the observation of normal processes.

"Secondly there is his description and analysis of mixed forms at various levels of those mental states in which different feeling elements are in operation simultaneously and with opposite and contrasted effects, so that the elements are set off one against another."

Harvard University C. A. SELZER

La Psychologie Organique. By PIERRE JEAN. Paris, Félix Alcan, 1925, pp. 297.

This, the first of three proposed volumes under the general title *Theorie de la Vie*, takes up the reactions of vegetables and of animals without a nervous system. The second volume will deal with "neural psychology," the third with "heredity." For the author all the functions of the most highly developed nervous system are not only potentially present in organisms without a nervous system, but are actually present for such organisms, in perfect form. Neural psychology is simply a specialized form of the psychology (organic psychology) which deals with the reactions of all the cells which are not nerve cells.

The main thesis of the book is that 'will' and 'choice,' in the sense of making a selection of possible responses in order to attain certain ends, are fundamental categories of a genuine psychological determinism. This 'will,' however, is not free but is determined by the needs of the organism. "Et le besoin, c'est la psychologie" (p. 271). In the author's opinion physical-chemical theories of tropisms, 'repetition,' functional excitation, tension, irritability, etc., either covertly imply sensitivity and orientation both of which are purely psychological or they patently controvert the facts.

A great many references to experimental results, some evidently original, ranging in complexity from Ray's culture of mushrooms in an agitated *milieu* to the effects of castration on horned animals, are cited to show that since the adaptations made by the organisms in such unusual conditions could not have been prepared for by natural selection they must be the result of volition.

The following typical quotations may serve to show the author's point of view: "The stalks, leaves, and grains of chlorophyll seek the light when it is useful, flee it when it is harmful. If organic psychology exists, the organs feel their needs and respond to them" (p. 54). "The chemotropisms and all the tropisms are instincts" (p. 71). "In case of an unusual need, vegetables look ahead (prevoient) and calculate" (p. 220). The action of leucocytes, the coagulation of the blood, the development of instincts, the metamorphosis of plants and animals, the regeneration of lost parts, the action of hormones, are all cited as cases which are inexplicable on any hypothesis that denies choice and orientation of the organism according to its own needs. "The ensemble of cells which by their multiplication and differentiation concur in the formation of an organ, constitutes a colony of living beings obeying a common instinct, just as a colony of termites construct their nest" (288).

Louisiana State University PAUL CAMPBELL YOUNG

The Psychology of Religious Mysticism. By JAMES H. LEUBA. New York, Harcourt, Brace & Co., 1925, pp. xii, 336.

Professor Leuba applies the term mystical to "any experience taken by the experiencer to be a contact (not through the senses, but 'immediate, intuitive') or union of the self with a larger-than-self." In the three forms of religious mysticism studied—savage intoxication, the Yoga system of India, and Christian mysticism—he has found emphasis on knowledge as an instrument for the suppression or enlargement of the self.

The impression of free, perfected life produced by certain drugs is pronounced comparable to the buoyant experiences of exaltation described by the Christian mystics. Ecstatic states and periods of dryness are traced through the lives of Christian saints, but their alleged oscillation is affirmed to be determined by physiological and psychological factors. The experience of Mlle. Vé, a modern mystic who recognized the sexual aspect of her revelations, illustrates the connection of mystic states with sex activity, particularly in persons deprived of normal satisfaction. In a survey of the lives of St. Catherine, Mme. Guyon, Santa Theresa and St. Marguerite Marie, passivity and asceticism are discovered to be methods for the furtherance of ecstasy. These mystics suffered from hysteria, but "moderate mysticism . . . is entirely free from that disease." Many mystics claim an Ascent of the Soul toward the highest stage, that of the trance, during which supernatural understanding, distortion of time and space, the appearance of brilliant light, an illusion of floating, and certainty of belief are experienced. Religious mysticism, it is contended, is a revelation not of God, but of man.

A statistical study shows that belief in the God of the established Christian religion is still widely prevalent among intellectual leaders in this

country, but that there is correlation between eminence and disbelief among these leaders. Historians and physical scientists profess greater belief, while biologists, sociologists and psychologists have less.

The book, following Professor Leuba's series of publications on the psychology of religion, is of especial significance. Its facts are drawn from reliable sources, including authoritative biographies of the saints. Quotations from religious and psychological documents support the statements made. Aside from its informative value, the volume merits commendation, for it brings mystical religion into the sphere of scientific research.

Cornell University FLORENCE B. WOOLSEY

L'Enfant Turbulent. By H. WALLON. Paris, Félix Alcan, 1925, pp. 649.

This book is based principally on two hundred or so observations taken by the author as the assistant to Dr. Nageotte, physician at Bicêtre, an asylum near Paris (boys), and at the Fondation Vallée (girls), from the records of children who consulted the author at the Dispensary for Nervous Diseases and at the Laboratory of Pedology, and from records obtained by Dr. Nageotte from his non-resident work at La Salpêtrière. The interpretation of the data reflects the author's experiences with cases of psychical and nervous troubles resulting from injuries to the skull and emotional upsets due to the World War.

The title of Dr. Wallon's book refers to a class of children displaying an excess of activity more or less pathological. Since the loss of a function by an adult does not reduce him to a condition equivalent to that in which he was previous to the acquisition of the function, Dr. Wallon considers it to be impossible for the most part to apply to the psychical troubles of children the psychopathic classifications used for adults. At most, in his opinion, the children would fall under the heading of psychopathic personality. Therefore he employs a psycho-motor classification for dealing with his child cases.

Dr. Wallon builds up composite pictures of the biological and psychical equipment of these children and treats their types of reaction as of primary importance on the basis that movement is indissolubly bound up with excitation in the elementary reactions and that the foundation of the higher processes of perception and ideation is the psycho-motor experience. He believes that associations, such as an idiot forms between his organic, affective, sensory impressions and his modes of reactions, are those retained incidentally by the normal child, but that in the case of the latter they do not constitute his total equipment nor do they attain the equivalent degree of refinement.

The book is divided into three parts: (1) The Periods of Psycho-motor Development; (2) Psycho-motor Symptoms; and (3) Observations. It also includes discussions of various investigations made by others and the application of their theories to the data of the author.

Harvard University ROXANNA BEEBE-CENTER

L'Activité psychique. By EDME TASSY. Paris, F. Alcan, 1925, pp. xvi, 126.

The book treats of a function which is the third of the activities designated by the author as constituting the whole behavior of the human organism. There is first organic activity, then mental activity, and lastly psychical activity. Psychical activity, as contrasted with the purely organic or with the sensory (mental), has to do with the reactions which the individual as a conscious personality makes to those excitations which he receives. The three chapters deal respectively with psychical sensibility, action of the self, and action among selves.

Harvard University M. B. PRATT

Clinical Psychology. By LOUIS E. BISCH. Baltimore, Williams and Wilkins, 1925, pp. xiv, 346, 17 pl.

The primary object of this book is, as the author states in the preface, "to give the teacher a working basis by means of which he or she may be able to recognize an atypical child in the classroom and to know how best to handle the situation." The book should be most valuable to all students of the subject.

The clinical field is comprehensively dealt with from this standpoint, and although the author puts forward no claims for originality, yet the background of his own experience is evident throughout. In the appendix (pp. 219-396) are 58 highly instructive case records selected as illustrative of the various deviations that may be encountered. The records cover the usual fields of examination and are well assorted, although it is to be regretted that the old Binet-Simon scale was used instead of the Stanford revision.

University of Arkansas A. RENDLE STONE

Fechner und Lotze (*Geschichte der Philosophie in Einzeldarstellungen*). By MAX WENTSCHER. Munich, Ernst Reinhardt, 1925, pp. 207, 2 pl.

The author confines himself rather strictly to summaries in chronological order of the theoretical content of all the principal works of Fechner and Lotze and to a short account of their lives; there is little effort shown to give more than just this. The book will be found quite useful, especially by those who, intending to study one or all of the works of Fechner or Lotze, wish a preliminary orientation in the philosophical ideas of these men. The psychologist will be disappointed, however, by the lack of attention given to the empirical and scientific rather than the speculative aspects of their works; the *Psychophysik* is treated in elementary fashion in five pages, and the *Medizinische Psychologie* has only six, which are devoted to metaphysical questions. Of the book 55 pages are given to Fechner and 129 to Lotze; the two articles are independent. There are two portraits and a bibliographical guide.

Harvard University FRANK A. PATTIE, JR.

Business Organization and Management. By HENRY P. DUTTON. Chicago and New York, A. W. Shaw Co., 1925, pp. xi, 545.

This is primarily a textbook for business managers. The value of psychology in training for business leadership is emphasized. McDougall's classification of instincts and modern psychopathology receive special and favorable attention.

Harvard University J. C. KELSON

Southern Pioneers. By HOWARD W. ODUM. Chapel Hill, N. C., The University of North Carolina Press, 1925, pp. iii, 202.

The content of this book, comprising the biographies of Woodrow Wilson, Walter Hines Page, Joel Chandler Harris, Booker T. Washington, and other southern notables of international repute, is primarily literary. Its incidental sociological and psychological import is, however, already being utilized by statistics-seekers in the field of leadership and genius. This incidental function affords its only scientific aspect, as there is no pretense at correlation with such anthropological, biological, eugenical, or even historical information such as the tilte might imply.

University of Michigan. MARTHA GUERNSEY

NOTES

Systematic and Scientific Study of Death in Man

It is a curious fact that as yet there seems to have been no systematic and scientific study of human death. There have been more or less sporadic efforts to make such investigations in exceptional and interesting pathological cases, but the regular order or the average manner of death is not known. Some of the main reasons for such study are:

(1) To gain trustworthy and general knowledge of what most often takes place in the dying hour.

(2) To learn the manner of death for different diseases, and the percent of those conscious and unconscious.

(3) To find in what percent of cases there is severe pain, slight pain, and no pain, and the nature of the pain if any.

(4) To note details in exceptional and special cases, the causes of these phenomena, and the length of dying time.

(5) To determine the characteristics and differences in dying of infants, children, adolescents, adults before middle life, during middle life, and in old age.

(6) To present eventually a general picture of the dying time, based upon a sufficient number of observations, and with instruments of precision when possible.

(7) That with such knowledge, the treatment of patients during the dying time may be more humane and delicately done.

(8) That in cases liable to be painful, proper precautions may be taken in advance, so that pain may be avoided or greatly lessened.

(9) That finally the knowledge of the dying time may be so increased that physical death will cease to be feared or dreaded, since through such knowledge pain may be practically eliminated and death may be made easy, gentle, and placid.

I am seeking knowledge upon death and shall consider it a great favor if any of the readers of the Journal, who makes observations and studies of cases during the dying time, will send me a copy of his notes and results. Due credit will, of course, be given when sufficient data are received to justify publication.

100 E. Capitol St. Arthur MacDonald
Washington, D. C.

Dr. Schoen on "The Elementary Courses in Psychology"

Having read with interest and in general approval, Dr. Schoen's article on the content and method of the elementary course in psychology,[1] I wish to offer a few comments upon what he said, and to add a criticism to those that he mentioned.

I am not so optimistic as Dr. Schoen about making psychology 'function' in the lives of the elementary students—assuming that the future continues to give us in our classes the same cross-section of humanity that we have been receiving. Any textbook which presents psychology scientifically must be difficult. It must demand thought and persistent intelligent study. Now, I feel, and I think many will agree with me, that at least half our students are incapable of perceiving the subtle relationships or formulations which any closely knit scientific work emphasizes. Indeed, if twenty-five percent of the beginning students obtain a real grasp of the subject and make it a part of their thinking-action habits, I feel that the

[1]Max Schoen, The elementary courses in psychology, this Journal, 37, 1926 593-600.

instructor has made a marked success. The sooner we cease fretting about the lower half the better will be the scientific content of our courses. If a book were proclaimed "as a book which easily makes all things in psychology clear to the average student" I should have the suspicion that the book was superficial. As for the bright students—well, they will get a good deal out of a course in spite of the teacher or the text.

I advocate the giving of a brief and 'surface-study' course in psychology in the high school which may serve as an introduction to the college work. If this were done we could then, as in physics and chemistry, turn in college to be serious and laborious business of presenting technical and scientific courses. But even if this were done, I should not feel optimistic about making psychology 'function' in the lives of our students. In order that this ideal may be accomplished, two conditions are necessary: first, the student must have a good mind; and secondly, he must be willing to pay the price by hard persistent study. Unfortunately, both of these conditions are fairly rare.

A criticism which Dr. Schoen neglected, and which is the chief point of this note, deals with the general practice in our colleges and universities of making the laboratory work in psychology optional. In chemistry, physics, zoölovy there is no choice; the student must take the laboratory in conjunction with the lecture course. Even the poorest colleges uphold this scientific ideal in respect to the teaching of these courses. But when we come to psychology how different is the case! Few departments of psychology require the laboratory work. I for one am firmly convinced that it is a mistake to grant the students this option; and in the institutions where psychylogy is counted as science credit the omission of the laboratory requirement is more than a mistake, it is a breach of faith, it is a violation of the well established scientific ideal.

Unfortunately there are many institutions of higher learning—not limited to the poor benighted colleges—where the student secures a 'soft science credit' by attending one lecture and two 'quiz' sections a week in psychology. I should like to see the day come when the American Psychological Association will go on record as recommending that no science credit be given in psychology unless the laboratory work accompanies the text-book course.

College of the City of Detroit E. B. Skaggs

The Eighth International Congress of Psychology

The eighth International Congress of Psychology, under the presidency of Professor G. Heymans, was held at the University of Groningen, Groningen, Holland, from Sept. 6-11. There were 242 names listed as members with about three-quarters of this number present. The meetings were held in the University buildings which were most admirably adapted to the needs of the Congress.

The Congress was in every sense an international one. The following is a listof the more prominent psychologist who were present: N. Ach, F. C. Bartlett, L. Binswanger, Ch. and K. Bühler, E. Claparède, O. Decroly, G. Ewald, A. Fauville, M. V. Gopalaswami, G. Heymans, E. Jaensch, P. Janet, D. Katz, F. Kiesow, O. Klemm, K. Koffka, W. Köhler, Mrs. C. Ladd-Franklin, G. R. Lafora, H. S. Langfeld, J. H. Leuba, A. Ley, M. Levy-Bruhl, J. Lindworsky, W. McDougall, A. Michotte, A. Meyer, E. Minkowski, C. S. Meyers, H. Piéron, W. B. Pillsbury, E. Rignano, F. Roels, E. Rubin, F. Sander, O. Selz, C. Spearman, E. Spranger, W. Stern, H. C. Warren, M. Wertheimer, H. Zwaardemaker.

The Congress opened on Monday, Sept. 6, with a reception in the University. The presidential address was delivered on Tuesday morning by the president, Professor G. Heymans. The program of the Congress was so

arranged that every morning was given over to a symposium. In the afternoons, the Congress was divided into two or three sections for the presentation of special papers.

The first symposium was on Intensity Differences of Sensations with papers by C. S. Meyers and H. Werner. The Psychology of Religion was the second of the symposia participated in by Pierre Janet, Ernest Jones, J. H. Leuba, and R. H. Thouless. The subject under discussion in the third symposium was *Verstehen und Erklären* with papers by L. Binswanger, Th. Erismann, E. Ewald, and Eduard Spranger. Kurt Koffka, A. Michotte, E. Rubin, F. Sander and C. Spearman contributed papers to the symposium on *Gestaltwahrnehmung*. The last symposium was on the subject, The Psychology of the Lower Races with topics by F. C. Bartlett, M. Levy-Bruhl, W. Meyer-Gross, and A. Storch. Of the symposia, the one upon *Gestaltwahrnehmung* seemed, as at the seventh congress at Oxford, to arouse the greatest interest, and, at the same time, the greatest antagonism. This meeting had to adjourn without giving opportunity to all those who wished to speak upon the subject.

The papers in the afternoon sections were devoted largely to theoretical and experimental topics. There seemed to be an absence of papers upon subjects in applied and educational psychology. In general, it may be said that many of the papers were of such a general nature, dealing with particular theories or with particular controversies, that the discussion was of a very general sort and very limited in extent. The question might well be raised regarding the programs of the International Congress whether it would not be better to have a selection made of the papers offered for the Congress, and thus materially reduce the number of topics presented. At this Congress there was an average of about twenty papers each day. With a fewer number of papers, and more time for each, it might be possible to discuss psychological problems without indulging in vague generalizations and personal antagonisms.

The Congress was organized and conducted in a most excellent manner under the direction of the first secretary, Professor F. Roels. The government of Holland and the city of Groningen coöperated in the arrangements so that from the time the border was crossed, one was graciously received and assisted. It was a true hospitality on the part of the national government, the municipality and residents of Groningen, and the members of the University.

A number of very pleasing social affairs were arranged by the local committee. On Friday evening, the municipality of Groningen tendered a reception to the members of the Congress at the *Stadspark* which was most pleasant, and to the American visitors at least, most memorable. The dinner on Saturday evening, with speeches from several members, brought the Congress to a close.

The following were present from the United States: Miss Olga Bridgman, Miss Psyche Cattell, David Fryer, H. M. Kallen, Mrs. Christine Ladd-Franklin, H. S. Langfield, J. H. Leuba, William McDougall, Adolph Meyer, L. J. O'Rourke, W. B. Pillsbury, Rudolf Pintner, M. S. Viteless, Seth Wakeman, H. C. Warren, and K. Zener.

Smith College SETH WAKEMAN

On Bluffing in Examinations

In the Summer School of 1926, at the University of Pennsylvania, a class of 29 showed evidence of more than the usual bluffing in a quiz. A trap was set for them in the next examination in an attempt to determine just how much and how frequently the college student would bluff. To this end, the class was asked to discuss ten terms devoting not less than one page to each. The seventh term in the list was made up out of the whole cloth—*Psychoterminality*.

Of the group of 29 men and women, only 2 made the positive statement that they did not know what the term meant. Six others left the question blank without a definite negative statement. The other 21 students made some sort of an answer ranging from one-half to three pages in length. Many of these answers showed a good deal of originality and most of them were based on an attempt to get at the etymology of the word. A brief analysis of the types of answers may be of interest: automatism, 1; vitalism, 3; prediction (of end), 4; to what end does mind exist, 2; resumé of psychological terms, 3; hypnosis, 2; behavior of lower organisms (lower end of animal scale), 2; the limits of psychology, 2; reaction time, 1; and causes, 1.

Do these results indicate that bluffing is as prevalent as this in all college groups or is it a beautiful example of what W. H. Burnham, of Clark University, would call a lack of 'intelligent ignorance.'

University of Pennsylvania S. W. FERNBERGER

INDEX OF THE FIRST THIRTY VOLUMES OF THE AMERICAN JOURNAL OF PSYCHOLOGY

An Index of the first thirty volumes of THE AMERICAN JOURNAL OF PSYCHOLOGY has just been issued (Dec., 1926). The Index is divided into four parts: I. List of volumes with their publication dates and editors; II. Original articles; III. Books reviewed; and IV. Subject matter. The contents of these volumes are thus made readily available.

Now that this beginning has been made the editors of the JOURNAL hope to issue a similar comprehensive index after the completion of every succeeding ten volumes. The burden of the present undertaking has been borne by many psychologists thereby making possible the preparation and publication of the Index at a moderate cost.

The price of the Index is low. Orders should be sent to D. R. Knight, Morrill Hall, Cornell University, Ithaca, N. Y.

K. M. D.

THE INDIAN JOURNAL OF PSYCHOLOGY

Since the publication of the October issue of the JOURNAL we have received the first and second numbers (dated January and April, 1926) of the *Indian Journal of Psychology*, which is published quarterly by the Indian Psychological Association.

The *Journal* is edited by a board of editors consisting of Owen Berkeley-Hill, European Mental Hospital, Ranchi; Haridas Bhattacharya, Dacca University; G. C. Chatterjee, Government College, Lahore; J. M. Sen, D. H. Training College, Calcutta; and N. N. Sen-Gupta, University of Calcutta. It is published in English so the work of our Indian confreres will be readily accessible. The articles in the first two numbers—which will be reviewed elsewhere in the JOURNAL—are of a quality that deserves attention.

K. M. D.

ERRATUM

Through the misreading of a sentence in some correspondence on the subject, I unwittingly misrepresented the amount of psychological apparatus manufactured annually by the C. H. Stoelting Company as stated on p. 583 of my recent article on the "Development of Laboratory Equipment in Psychology in the United States" (this JOURNAL, 37, 1926, 582-592). It turns out that the figure $500,000 covers apparatus from all sources, including foreign markets, and also test-material published by book concerns in this country. Of this business the Stoelting Company receives, of course, its share.

University of Iowa CHRISTIAN A. RUCKMICK